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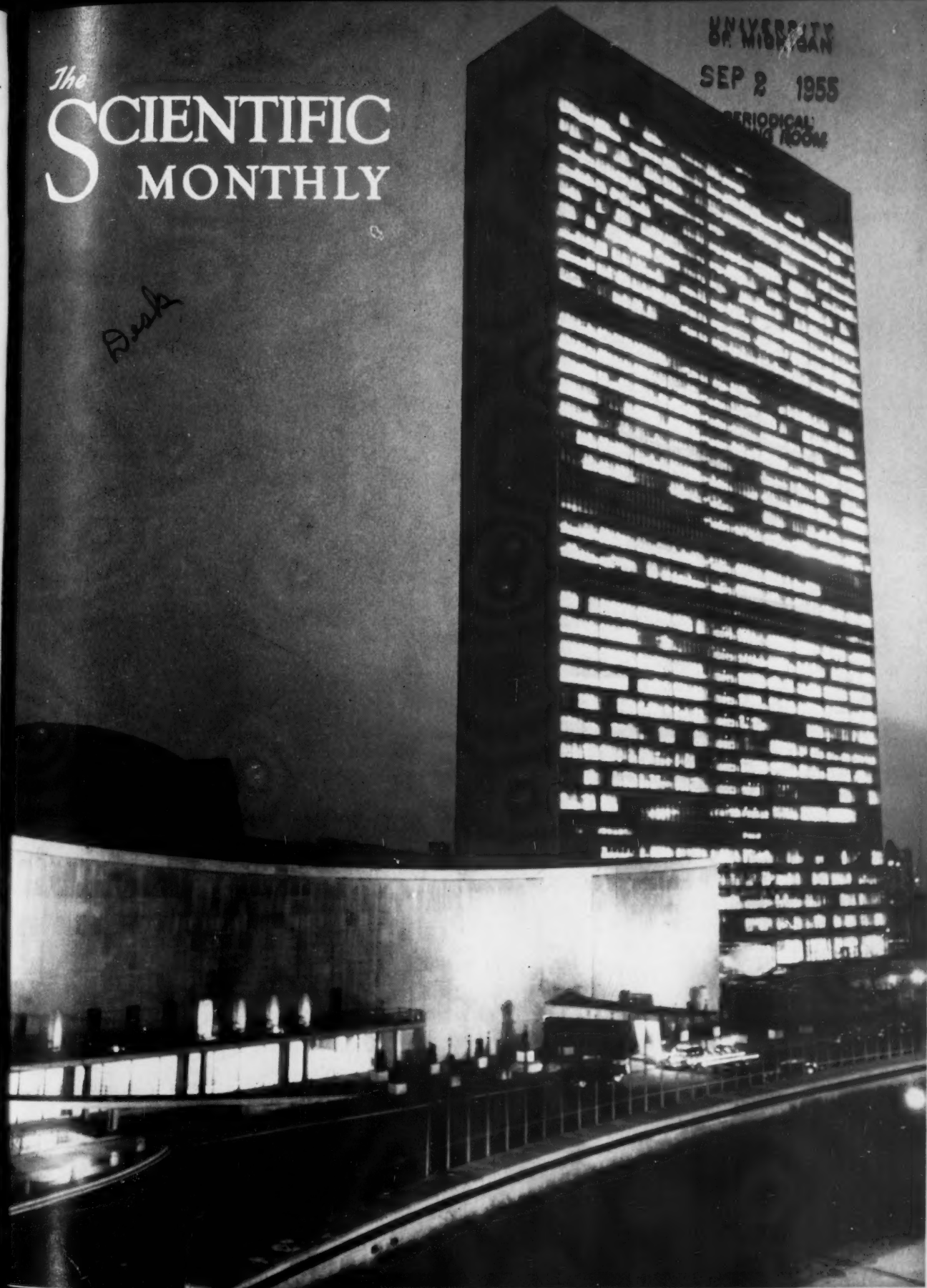
The
**SCIENTIFIC
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UNIVERSITY OF MICHIGAN

SEP 2 1955

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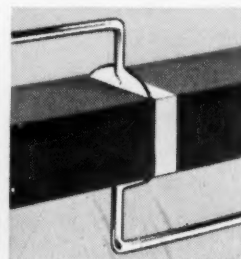
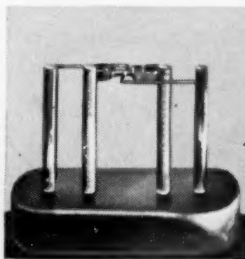
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THE SCIENTIFIC MONTHLY

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SEPTEMBER 1955

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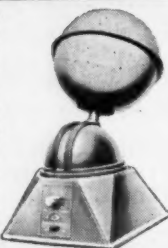
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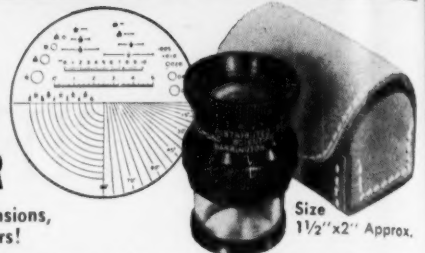
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THE SCIENTIFIC MONTHLY

SEPTEMBER 1955

Improving Science Teaching

AAAS COOPERATIVE COMMITTEE ON THE TEACHING
OF SCIENCE AND MATHEMATICS

The AAAS Cooperative Committee on the Teaching of Science and Mathematics includes one member for each of the following: American Association of Physics Teachers; American Astronomical Society; American Chemical Society; American Institute of Physics; American Nature Study Society; American Society for Engineering Education; American Society of Zoologists; Botanical Society of America; Central Association of Science and Mathematics Teachers; Division of Chemical Education of the American Chemical Society; Board of Directors of the AAAS; American Geological Institute; Mathematical Association of America; National Association of Biology Teachers; National Association for Research in Science Teaching; National Council of Teachers of Mathematics; National Science Teachers Association; Section Q (Education) of the AAAS; and (by invitation) the Academy Conference.

ONLY 249 men and women who had prepared to teach high-school physics graduated from colleges and universities in the United States this spring. And only half of these few graduates—if recent experience serves as a guide—will actually be teaching high-school classes next fall. The other half will have been diverted into other occupations by more attractive employment opportunities, will be in military service, or for other reasons will not be teaching.

One hundred twenty-five new physics teachers are clearly far too few to replace those lost through death, retirement, and resignation from the nation's 25,000 high schools and to teach the additional classes called for by an enrollment that is already well above the 6 million of 3 or 4 years ago and that is expected to reach 11 or 12 million by 1965.

The widening gap between supply and demand is only in part due to the decline in the size of college graduating classes since the peak of 434,000 in 1950. During the past 5 years the total number of graduates has declined 39 percent, but the number

prepared to teach high-school physics has dropped 74 percent.

Although the pinch is most severe in physics, a similar situation prevails throughout science and mathematics; the supply of new high-school teachers has declined more rapidly than the total number of college graduates.

High-school principals, faced with an inadequate supply of science and mathematics teachers, have two courses of action open to them. They may use teachers who are inadequately prepared or they may drop courses that their students want and should have. Either action will mean that fewer students in the years ahead will enter college with a developing interest in science and mathematics.

Consequences of the Shortage

The shortage cannot help but affect unfavorably the rate of production of future scientists and the quality of their training; many students develop their interest in scientific careers at the high-school level. Also adversely affected will be the scientific

knowledge and appreciation of the general public; many students are formally introduced to the sciences during their high-school years, and for a large number, high-school courses represent their only formal study of science.

The shortage of teachers of science and mathematics poses such serious consequences for scientific and technologic progress in the United States that an immediate, coordinated, large-scale attack on the underlying causes seems necessary. Since all branches of science will be affected, science as a whole has an important stake in the improvement of high-school teaching. The AAAS—first through the Cooperative Committee on the Teaching of Science and Mathematics and the Academy Conference (the composite organization of state and city academies of science), and then through the endorsement of the board of directors—has decided to do what it can toward improving the quality of science instruction and increasing the number of well-trained teachers. The program, *Science Teaching Improvement Program*, developed by the cooperative committee, will form the basic plan, and the Carnegie Corporation of New York has generously provided a grant of \$300,000 to help finance these efforts. Additional funds will be necessary to carry out fully the proposed program.

Factors in the Current Situation

Of the factors responsible for the shortage of well-qualified teachers of science and mathematics and the deficiencies of much of the instruction given in these fields, four seem to be particularly important. Although some of these factors affect all high-school teachers, their combined effect has been greater on teachers of science and mathematics than it has on teachers of other subjects.

1) *Birth rate changes.* High-school enrollment is increasing and will continue to increase. The upsurge of births in the United States during World War II was followed by even higher birth rates in the postwar years; births in 1940 totaled a little more than 2 million; in 1954, 4 million. But the newly graduated teachers for the expanding high-school population must be drawn from the thin generation born during the 1930's when birth rates were low. Students currently graduating from college are wanted for many occupations other than teaching; the over-all shortage of new graduates affects teaching as well as other occupations.

2) *Lower salaries of teachers.* The salaries offered to high-school teachers are frequently lower than those offered to the same individuals by other prospective employers. Moreover, the salary increases and ultimate salary ceiling to which a teacher can

look forward are lower than those in other professional fields.

3) *Educational policies and attitudes.* During recent decades the high school has changed from an educational institution designed chiefly to train a few students for college admission to one designed to give terminal training to large numbers of students who are not going to college. Although this change has brought about many advantages, it has also made it more difficult for most high schools to give rigorous, high-quality courses in the subjects that are most appropriate as college-preparatory work. Consequently instruction in science and mathematics has suffered.

4) *Attitudes of scientists.* Although many scientists criticize the high school, and many college teachers of science deplore the preparation their students bring from high school, scientists have not, on the whole, accepted responsibility for the training of high-school teachers. This responsibility has been left largely to departments of education. College departments of science have not seen to it that prospective teachers had a good background in subject matter; they have not provided—as have education departments—summer-school courses for teachers; they have not encouraged their students to become science teachers; they have not made science teachers feel themselves to be part of the total scientific community. The many individual exceptions to these generalizations are encouraging, but still leave it true that scientists themselves must accept part of the responsibility for the shortage of science teachers and the inadequate preparation of many who are teaching science courses.

The AAAS clearly cannot rectify all the defects, but it can help. It has selected the following seven programs or types of effort, and it hopes to make useful contributions to each. Although these seven projects have been selected as desirable lines of effort, it is likely that future conditions and decisions will modify many of the details outlined here, and that they may produce major changes in the projects currently planned.

Responsibility of Scientists

High-school science teachers should have reasonable knowledge of the subjects they teach. There is room for debate over what constitutes "reasonable knowledge," but there seems little question that many individuals now teaching science in the high schools are inadequately prepared in the subject matter of science.

Educators generally believe that all high-school teachers should have a reasonable background in

the field of teaching. Again there is room for debate over the proper amount; most states require between 18 and 30 semester-hours in such areas as student teaching, child psychology, teaching methods, and the history of education. Scientists are by no means unanimous, but many agree that such courses constitute desirable preparation for the prospective teacher.

State certification requirements and departments of education usually see to it that beginning teachers meet the formal requirements in education. Over the country as a whole there is no comparable insistence upon adequate subject-matter preparation as it is defined by scientists. It is in this area that we think scientists can and should accept greater responsibility and exert greater influence.

In the typical college or university science department, attention has been concentrated on the preparation of students for graduate work and research careers or on the preparation of students for engineering, medical, or other applied science areas. Students with an interest in high-school teaching and with the necessary aptitudes in science and mathematics either have not been encouraged to prepare for teaching or have been discouraged from making such preparation.

How many individuals who might have become satisfactory teachers of high-school science and mathematics have been lost to teaching in this way cannot be calculated. Whatever the past losses, if the situation is to improve, collegiate science departments must actively encourage qualified and interested students to prepare for careers in teaching, both high-school and college, but especially high-school. Accordingly, the AAAS plans an organized effort to bring the facts concerning the critical shortage of high-school teachers of science to the attention of college and university departments of science and mathematics and to urge their more active participation in the recruitment, training, and encouragement of high-school teachers of science and mathematics.

What is appropriate on one campus may not be appropriate on another. The following list, therefore, includes what appear to be desirable activities, but the details must be expected to differ from one institution to another.

- 1) Collegiate departments of science can examine, and frequently improve, their undergraduate courses and major requirements from the standpoint of their appropriateness for future high-school teachers.

- 2) Working with departments of education and state school officials, they can revise certification requirements to place greater stress on subject-matter preparation of prospective teachers.

- 3) They can develop courses suitable for high-school teachers who return to the campus for summer work. In many states a teacher with graduate work or a master's degree qualifies for a salary increase. The undergraduate work of many teachers who would like to get such increase is not adequate, however, for enrollment in the traditional graduate courses in science and mathematics. Turned away by departments of science, they concentrate in education, in which they can receive graduate credit. This situation creates a problem for science departments: they do not wish to water down their advanced courses; neither do they wish to give graduate credit for their elementary courses. Yet unless they make some adjustment, they are missing an opportunity to raise the level of high-school teaching and improve the preparation of future students in their own fields.

A number of colleges and universities are meeting this problem by developing special courses, usually offered in the summer term, that are open only to teachers. Thus these courses do not interfere with the usual sequence for students with other interests but are valuable for high-school science teachers. On some campuses these courses carry graduate credit in science; in others they are counted as education credit, even though they are planned and taught by the faculty of science departments.

Other adjustments are also possible: a master's degree in science teaching can be given without interference with the usual master's degree in science. Efforts can be made to have school regulations amended to allow salary increases for appropriate additional work, even through a good portion of the work is at the undergraduate level.

- 4) College and university departments of science and mathematics may assist high-school teachers in other ways, for example, by providing a departmental staff member to a neighboring high school to offer advanced instruction in science for a selected group of students. Sponsorship and support of meetings and conferences at which college and high-school teachers may exchange information is still another avenue through which college scientists can assist in improving high-school science teaching. Representatives of college and university staffs might also be made available for consultative and lecture services to high schools.

Emergency Measures

A large potential source of high-school teachers of science and mathematics consists of individuals who have had college work in these fields, who may be interested in teaching, but who lack the required courses in education. Such individuals are found

among seniors in liberal arts, engineering, pre-medical, pre-dental, and other curriculums; some of the students who started to specialize in these other fields later developed an interest in teaching, but made that change too late in their college careers to take the usual sequence of courses in education without unduly lengthening their college programs. Similarly, among college graduates with substantial amounts of work in the sciences and mathematics are some who would like to teach.

Special accelerated programs in education should be arranged for senior undergraduate students who wish to qualify for teaching positions before the beginning of the next academic year. For students in independent liberal arts colleges without departments of education, cooperative arrangements with departments of education in nearby institutions may need to be worked out. In any case, institutions of higher education should take the initiative in setting up such accelerated programs and in bringing them to the attention of interested students.

Many states provide for emergency teaching certificates that make it possible for a partially qualified individual to obtain immediate teaching employment and to satisfy the requirements for a standard teaching certificate while employed. In some cases accelerated programs in education leading to emergency certification may be possible; in others, especially those found among college graduates out of school for some years, supplementary or refresher work in science may be more appropriate. Colleges and universities, in cooperation with certification authorities, can take the initiative in establishing such programs and in bringing them to the attention of interested individuals in the regions that they serve.

The AAAS plans to study the effectiveness of tapping these resources of potential science and mathematics teachers, to collect information on what is already being done toward that end by individual institutions, and to hold a series of state conferences of scientists, educators, and state certifying officials to stimulate additional efforts toward the development of emergency programs for the training of science and mathematics teachers.

Recruitment for the Future

The efforts described in the preceding section are required to meet the pressing current shortage of science and mathematics teachers. To satisfy expanding requirements for the future, vigorous measures will be necessary to interest a considerably larger number of potentially qualified students in preparing for teaching careers.

Many steps may be taken toward the accomplishment of this objective. Among these are (i) the preparation and dissemination of appropriate guidance materials on mathematics and science teaching, (ii) the promotion of vocational guidance programs through assemblies, radio, and television; (iii) the utilization of scientists and engineers as counselors of students with scientific interests; and (iv) the encouragement of high-school science clubs, science fairs, and junior academies of science.

An important element in the development of a recruiting effort is knowledge of what it is that people find attractive and unattractive in the field for which one is recruiting. Some of these factors are already known insofar as they concern the field of teaching, but current and better information is desirable. Consequently the AAAS plans to make a study both of the factors that attract people into teaching, and of the factors that are important in influencing teachers to turn to other kinds of work. The information from the study can be used, not only in guidance and recruiting, but also, to some extent, in suggesting changes in school policies and arrangements that would make teaching more attractive.

Higher Salaries

At the root of much of the difficulty of attracting and retaining competent teachers are the prevailing low salary scales and the deterioration in the relative economic position of teachers with respect to other occupational groups. Although all teachers are affected by these economic factors, the problem arises most acutely in the recruitment and retention of science and mathematics teachers. Industry and government compete more aggressively for persons with training in science and mathematics than they do for prospective teachers in other fields.

We support the principle that beginning salaries, rates of salary advance, and salary ceilings for teachers should be comparable to those available to other professional personnel of equivalent training. Obviously the AAAS cannot bring about such a sweeping change; this can be accomplished only by widespread local action at the community level. What the AAAS can do is to enlist the aid of state academies of science and other state and local scientific groups in bringing to clearer public attention the need for higher salaries for teachers and the special problems that exist in the fields of science and mathematics. Moreover, its 50,000 members and the members of its 260 affiliated and associated societies could lend their influence to these efforts in their own communities.

The salaries of teachers of science and mathematics are usually controlled by general salary schedules. It is doubtful that salaries of science and mathematics teachers could be raised above general levels, and debatable whether they should be. Efforts to increase the total income of science teachers are, however, being made by methods other than salary increases. Therefore a study is also contemplated of the various ways in which science teaching can be made more attractive financially by such devices as year-round employment, summer employment in science-related industries, or additional pay for directing student research projects, science clubs, science fairs, and other activities. Most salary schedules at present do not provide for increases on a merit basis. Although it is recognized that such differential scales are debatable, consideration of this problem by scientists might lead to a more satisfactory solution.

Better Working Conditions

To a considerable extent, the large size of classes, heavy teaching loads, and lack of adequate laboratory facilities and instructional equipment discourage competent students of science and mathematics from looking forward to careers as teachers. These same factors contribute to the high rate at which teachers of science and mathematics leave teaching for other occupational fields.

The AAAS, both as an association and through its individual members, can bring to the attention of appropriate groups the need for improving the conditions under which science teachers work. It will investigate the effectiveness of the use of teaching assistants and of such instructional aids as motion pictures, radio, and television in increasing teaching efficiency and providing the teacher with more attractive working conditions. It will give special attention to the adjustment of teaching load, so that a more effective job may be done, particularly in connection with laboratory instruction.

Believing that closer affiliation with organized science and the resultant enhancement of professional *esprit* would benefit teachers, the AAAS plans to encourage the attendance of teachers at scientific meetings and will support the provision of time off and reimbursement of travel expenses to encourage such attendance.

Awards for Distinguished Teachers

In recent years the secondary-school teacher has not enjoyed high prestige, not by any means, we think, as high as his contribution to society merits.

The public recognition of exceptionally able teachers of science and mathematics represents one means of enhancing their prestige. The AAAS therefore plans to institute an annual program of awards to outstanding teachers. The teachers to be honored will be those who, over a period of years, have been recognized in their schools and communities as exceptionally effective, whose knowledge of science or mathematics approximates that of the master's degree level, and who have, through writing or other means, been of substantial help to their fellow-teachers. Such teachers are good "professionals" and merit higher prestige than is accorded to teachers generally. We propose to honor them with citations as Distinguished Service Teachers. Since these citations are intended not only to reward excellence but also to call public attention to the importance of good teaching, the citations will be awarded in the teachers' own schools.

If financial backing can be secured, even more might be done. For example, the teachers selected for citation might be given monetary awards; or the expenses might be underwritten for each year's group to attend the annual meeting of the AAAS.

The scope of these plans is flexible. The number selected each year should be small enough to make the citation a real honor, yet large enough to make the motivation and prestige values as widely effective as possible. Perhaps 100 Distinguished Service Teachers a year would be a good starting level.

Intelligently administered, rank and honors are not only an award to those who receive them but an inspiration to those who aspire to them. For many individuals, and particularly those who are sincerely attracted by the opportunity to guide the intellectual development of young people, the respect accorded the teacher may provide the best measure of the value that society places on teaching.

Consultants to Teachers

The plans described here are designed to retain experienced science teachers in the classroom and to increase the number of young people who prepare to teach science. Even if these goals are achieved, the greatly increased high-school enrollment of the next few years will in all probability necessitate the use of many science teachers with less than adequate preparation. It is proposed, therefore, to undertake a pilot study of a method for "upgrading" the work of relatively inexperienced and inadequately prepared teachers.

The plan provides for the employment in each of several geographic regions of two competent

science or mathematics teaching counselors—expert consultants—who will tutor, assist, and serve as a source of information and help to the less-experienced and less-competent science teachers of the region. These consultant teachers would have no administrative supervision over their colleagues and would be employed only in regions in which supervisory help in science and mathematics is not already available within the school system.

If one such consultant were made available to each group of 20 to 25 teachers, the increase in staff would amount to only 4 or 5 percent. The number of teachers will increase anyway; perhaps this type of increase would be more effective than others. It seems worth while to test the hypothesis that the total effectiveness of instruction will be greater with such consultants than if the same individuals simply taught classes all day.

If this hypothesis is borne out, it is hoped that the demonstration will encourage school systems, state departments of education, and colleges and universities to assume permanent responsibility for providing continuing consultant services in science and mathematics to nearby high-school teachers of those subjects.

Role of the AAAS

It should be obvious that the AAAS can work more effectively on some of the foregoing proposals than it can on others. On the one hand, the AAAS has strategic opportunity to work toward the assumption on the part of scientists of greater responsibility for the training of science teachers. On the other hand, there is nothing unique that the AAAS can do on the problem of raising teachers' salaries.

There are so many facets to the problem of bringing about a sizable increase in the supply of well-prepared high-school teachers of science and mathematics, and of improving high-school teaching in these fields, that the AAAS cannot hope to achieve any large measure of success without the concurrent efforts of many other groups and organizations. Although it will supplement and sometimes cooperate with other programs looking toward the same ends, the AAAS will concentrate its major effort on the projects that it is particularly well qualified to carry out by virtue of its broad representation of scientists and science teachers in all the branches of all the sciences at all levels.

Education is the acquisition of the art of the utilization of knowledge. This is an art very difficult to impart. Whenever a textbook of real educational worth is written, you may be quite certain that some reviewer will say that it will be difficult to teach from it. Of course it will be difficult to teach from it. If it were easy, the book ought to be burned; for it cannot be educational. In education, as elsewhere, the broad primrose path leads to a nasty place. This evil is represented by a book or a set of lectures which will practically enable the student to learn by heart all the questions likely to be asked at the next external examination.—A. N. WHITEHEAD.

Climatic Contrasts in the United States

STEPHEN S. VISHER

Dr. Visher, professor of geography at Indiana University, received his doctorate from the University of Chicago. He has contributed 14 articles on climate to The Scientific Monthly (1922-1947) and many others to more technical journals. This article gives some of the disclosures and deductions from a few of the 1000 maps contained in his recently published Climatic Atlas of the United States and some sample maps therefrom.

ONE of America's assets is its possession of several climates, each of which has some special advantages for some particular human activity. The location of the United States on the globe puts nearly all of it in latitudes which have distinct seasonal contrasts and also generally sufficient summer warmth for agriculture. The 30th parallel, often considered to mark the boundary between middle latitudes and low or tropical latitudes, goes through New Orleans. Fortunately, the United States extends southward into the warmer latitudes in Florida and southern Texas. Although northernmost "continental United States" (latitude 49°) is well south of the northern limit often given for middle latitude (60°), the cold which is characteristic of high latitudes extends into the United States in winter, especially on the mountains and plateaus.

The number of climates in the United States depends on the bases used in defining them. One expert divided California alone into more than 15 climates, about twice as many as some other experts recognize for the entire country. Eight climatic regions are readily distinguishable: three interior ones, four coastal, and "high mountains." The vast interior may be subdivided into humid continental, semiarid continental, and arid. The zone between the humid and semiarid is sometimes called the subhumid. The southeastern coastal type is sometimes called the humid subtropical, while the southwestern coastal type is the dry-summer subtropical, or Mediterranean, here often called the California type. The northwestern coastal climate, that of western Oregon and Washington and northwestern California, is often called the marine or British type. The coastal type on the northeast is narrow and similar to the humid continental but has less severe cold, less snowfall, and cooler summers, but a longer season without killing frosts.

Of these lowland climatic regions, the smallest and least distinct is the northeastern coastal one, and the largest is the humid continental. Two-thirds of the people of the United States live in

these two types of climate. The arid and semiarid continental types support few people. The humid subtropical type, which extends from about Washington, D.C., south into Florida and west along the Gulf Coast into eastern Texas, is moderately densely peopled. The California type includes only a part of that vast state, other parts of which are arid, semiarid, marine, or high mountain. Mountain climates are present locally in various other parts of the West, and to a minor extent also in the East.

Within each of the major climatic regions there are minor subdivisions, associated with differences in elevation, slope, and exposure to the influences of lakes and other features of the surface, including soil and vegetation.

Climate includes much more than average temperature and rainfall, the attributes which are most commonly considered. With regard to temperature, the seasonal contrasts, the day-to-day and day-to-night variation, the severity and frequency of hot spells and cold spells, and the general reliability or regularity are of real significance, as is the length of the season without killing frosts, the so-called "growing season."

With regard to precipitation, seasonal and day-to-day contrasts are also important, and likewise how much falls as gentle rain, as downpours, as snow, hail, or sleet. The severity of drouths and of periods of excessive precipitation has special importance. Sunshine, cloudiness, and foginess are other significant aspects of the climate. With regard to the wind, its direction, velocity, and fluctuation are significant, as are windstorms. The humidity of the atmosphere varies widely from place to place and from time to time and should be considered, as should the rate of evaporation. Variations in the amount of air, and its pressure play a major role in causing the winds and have various other effects, especially at higher altitudes.

Temperature contrasts. The average January temperature is about 60°F in central Florida, 30° in Boston, New York, Cincinnati, and St. Louis

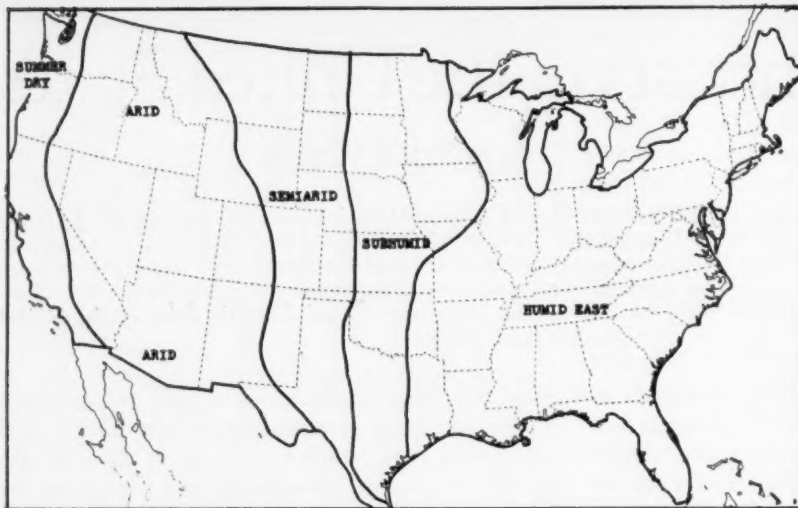


Fig. 1 Generalized precipitation regions in the United States.

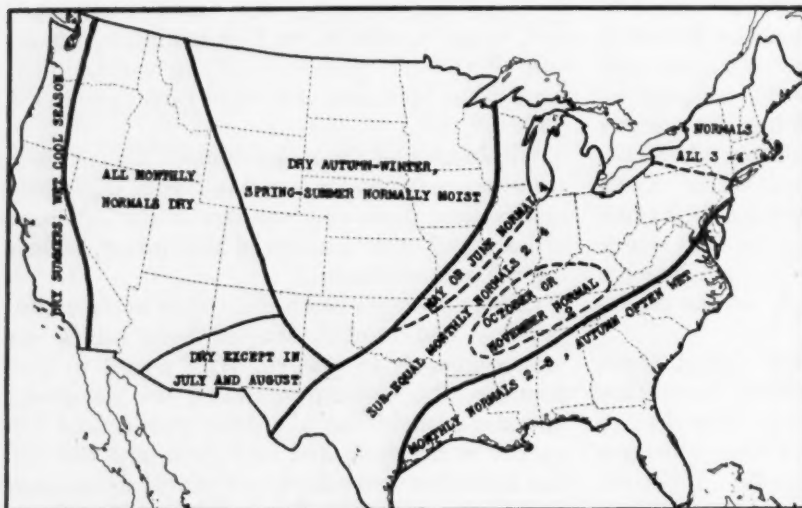


Fig. 2. Regions based on seasonal distribution of precipitation.

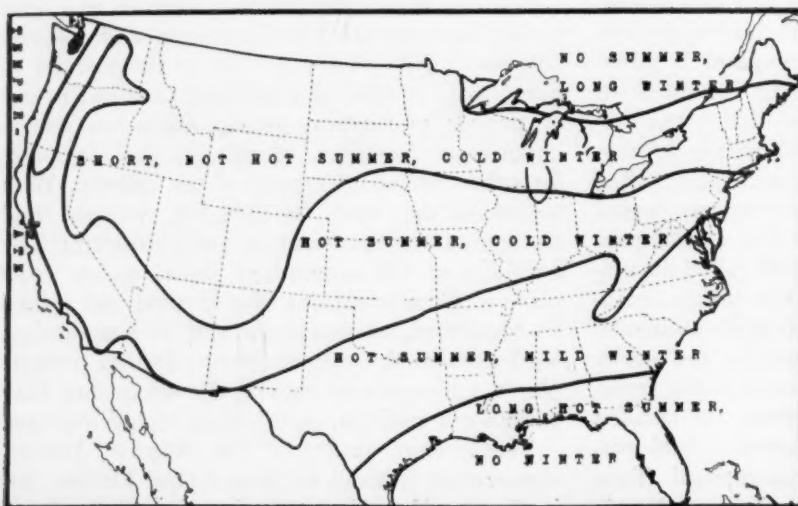


Fig. 3. Temperature regions based on seasons.

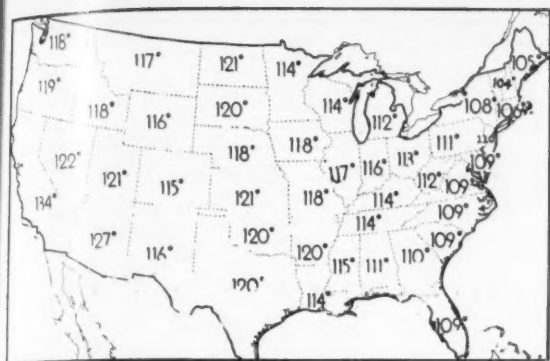


Fig. 4. Highest summer temperature officially recorded in each state, including 1954.

but less than 10° in northern Maine, Minnesota, and North Dakota. The contrasts between zero as the average for January in northern North Dakota and 68° in southern Florida and 52° in southwestern California help to explain the winter attractiveness of the latter places.

During the average winter, the temperature drops occasionally to -30° or lower from Idaho to Wisconsin, and to zero from Rhode Island to southwestern Texas, but only to barely freezing in central Florida and southwestern California.

In an exceptionally cold winter, the temperature occasionally falls below -50° in northern New York and northern New England and in an extensive belt from Lake Michigan west to eastern Washington, a total of 16 states. Minus 60° has been officially recorded in North Dakota, Montana, Idaho, Colorado, and Wyoming. The lowest three records are -69.7° (Rogers Pass, Montana, 20 January, 1954), -68° (Montana, February), -66° (Wyoming, February). It has been -59° in Minnesota, and -52° in northern New York. Temperatures of -30° or colder have been recorded in Tennessee, West Virginia, and Kentucky, and -35° or lower in Indiana, Ohio, and Pennsylvania (-39° in both Ohio and Pennsylvania). Of the 48 states, only two lack official records of -16° or colder (Florida -2° , and South Carolina -13°). Frosts have extended as far as Key West, Florida, and Brownsville, Texas, and over all the country except a bit of extreme southern coastal California.

The coldest quarter of the year generally ends in the first week of March, but if spring is defined as the season when the daily normal temperatures rise to 32° , it arrives in February in a zone that extends from Cape Cod to Utah. Mild spring—(day and night averages of 50°), arrives usually in February near the Gulf of Mexico, in April in a wide zone from New York to Utah, but not until May to the north of that belt. Summer (daily

normals above 68°) comes in March to much of Florida, in April elsewhere in the Deep South, in May from Maryland to Missouri, and in June from New York to South Dakota. Hot summer (normals above 75°) commences in May in the Deep South, and in June from Virginia to Missouri. Most of the North and West does not have average day and night temperatures as high as 75° .

The average date of the last killing frost in spring is in February in northern Florida and elsewhere close to the Gulf of Mexico. It occurs in March throughout much of the Deep South and near the Pacific and in May in most of the northern half of the country. Killing frosts have occurred, however, as late as early April in northern Florida, and in June in much of the North. The first autumn killing frost comes usually in September in the North, in October in most of the country, and in November in the South. The season usually without killing frosts varies in length from more than 300 days close to the Gulf, 200 to 300 days in the rest of the Deep South, and from 120 to 180 days in most of the remainder of the country, except in the mountains. In the shortest growing season of 40 years, it was, however, less than 120 days in a wide northern belt and was less than 200 days in about half of the Deep South. Cotton, which requires 200 frost-free days, is occasionally badly damaged by frost even in the Cotton Belt.

Most cold spells are brief in the South, where the lowest daily normal temperatures are above 50° in the Deep South, and about 40° in the Upper South. Frost occurs in about one-fourth of all days, however, in a belt extending from Boston to Cincinnati, St. Louis, and El Paso. Frost occurs during about one-third of all mornings from interior Massachusetts to Cleveland, Chicago, and central Kansas. It occurs during more than half

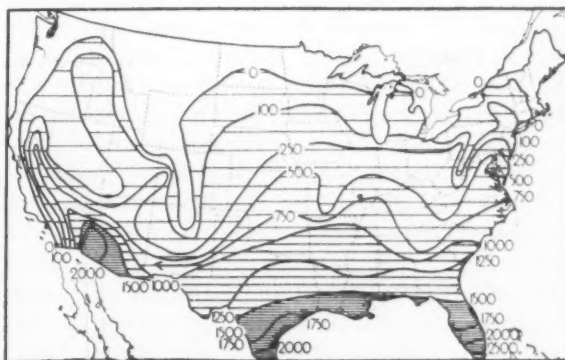


Fig. 5. Contrasts in the need for artificial cooling of buildings: annual average total hot-degree-day-units (sums of amounts by which the average temperature of each day with an average above 70° exceeds 70°).

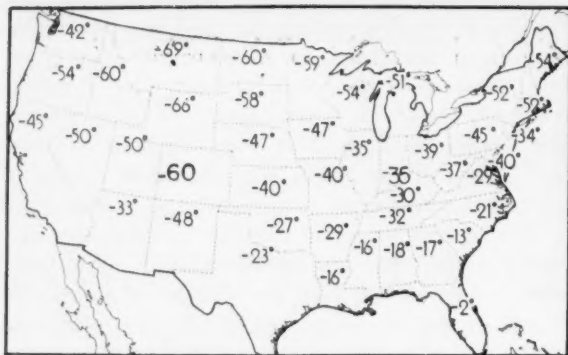


Fig. 6. Lowest winter temperature officially recorded in each state, including 1954.

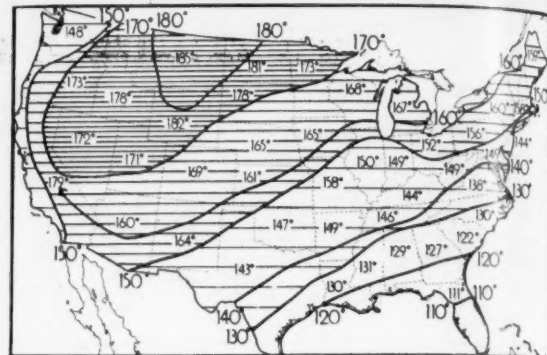


Fig. 7. Annual absolute range of temperature, based on state high and low records to 1945.

of the mornings in a large region from North Dakota and Montana to New Mexico.

The normal July temperature (average of day and night) is from 80° to 85° in roughly the southern third of the country, from 70° to 80° in about the middle half, and less than 70° in less than a fourth of the country, at the North, in high altitudes, and near the Pacific. Temperatures above 100° occur at least once each normal summer in about half of the country but not in the more populous northeastern one-fourth, north of a crooked line from Virginia to Dakota. Temperatures of over 100° have been experienced, however, in every state; and 109° or higher in all but New York and New England. The highest record for states east of the Mississippi River is 117° in Illinois and 116° in northwestern Indiana. West of the Mississippi 16 states including North Dakota, Montana, and Washington have had over 116° , and Arizona and California more than 126° . The record is 134° in Death Valley, California, 10 July, 1913.

The duration of hot weather varies widely. For example, a day-and-night average temperature in excess of 75° prevails for more than half of the year in southern Florida, for more than a third of the year in the rest of the Deep South, but for less than 50 days north of a line through Baltimore, eastern Kentucky, southern Iowa, and central Kansas. The region north of New York City, Cleveland, Chicago, and Sioux City usually lacks such hot weather. Day-and-night averages in excess of 84° are normal in July in much of southern Texas and in southwestern Arizona and adjacent California. No normal night is constantly hotter than 70° except in the southeastern part of the country, south of a line from southern Virginia to southern Illinois and southeastern Kansas, and thence south to southwestern Texas, and in the Imperial Valley and Death Valley, California and part of Arizona.

There are 60 such nights per average summer south of South Carolina and Oklahoma, and 100 in most of Florida and southeastern Texas. The sums of the daily temperatures above 70° yield what has been called the "hot degree-day-units," useful in revealing contrasts in the cooling requirements of buildings. In a normal year most of the North and West lack these units, but south of a curved line from Washington, D.C. to central Kansas there are 500 units or more; in most of the Deep South there are 1000 units or more; southern Florida and southern Texas have 2000. Thus, there are wide regional contrasts in the amount of cooling needed if indoor temperatures are to be kept "not excessive."

The relative heating requirements for buildings vary in roughly the reverse direction of the cooling requirements, but the regional contrast is much less. As has already been noted, if the need is based on the number of days with day and night average temperatures above 70° , much of the North and West requires little or no cooling in the average summer; while in the Deep South, weeks of such cooling are desirable. With regard to heating needs,

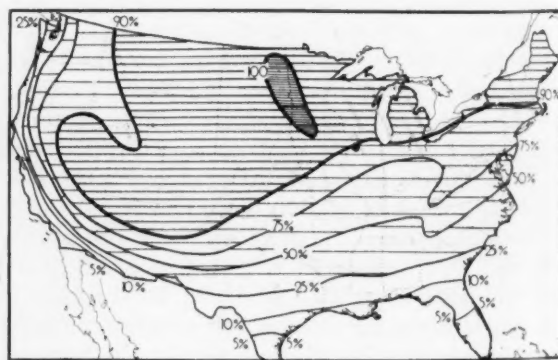


Fig. 8. Percentage of January nights with minima of 32°F or lower.

Fig. 9. Regions based on frequency and severity of freezing: 1, frost rare; 2, freezes rare; 3, soil usually freezes 1 to 4 in. annually, freezes frequent; 4, soil normally freezes 6 to 18 in., frosts occur 5 to 7 mo; 5, soil normally freezes 18 to 36 in., only 3 or 4 mo without frost; 6, soil usually freezes 3 to 6 ft., frosts in all months, or all but one or two.

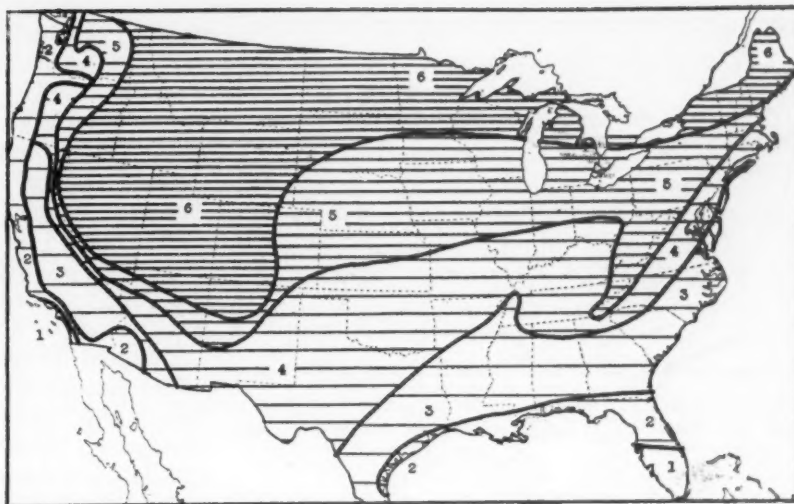


Fig. 10. Contrasts in the need for artificial heating. Figures are the annual total number of cold-degree-day-units, sums of totals below 65°F.

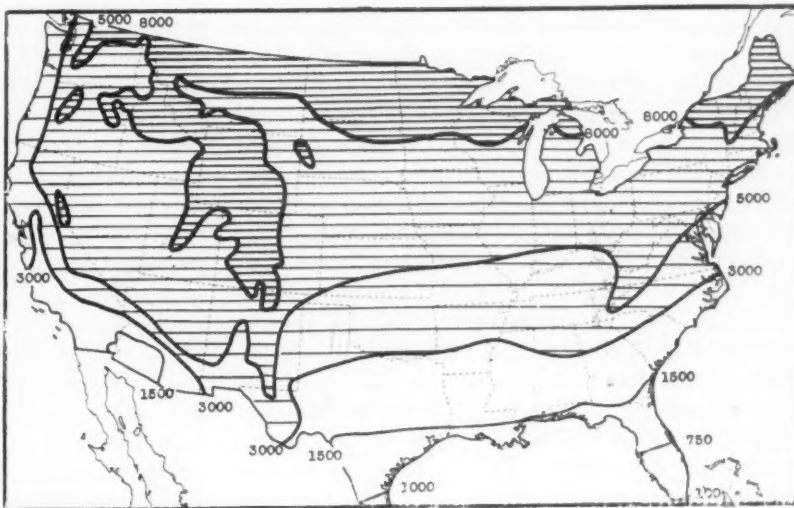
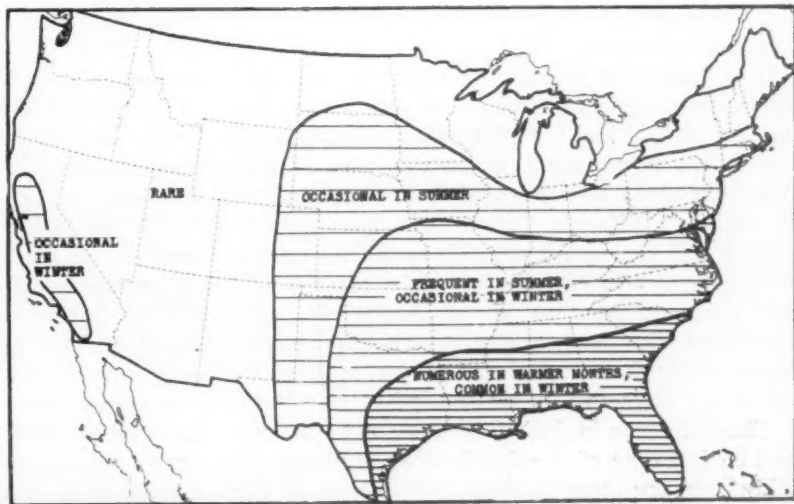


Fig. 11. Precipitation regions based on the frequency of "excessive" rains. Excessive is vaguely defined but includes 1 in. in 30 min, 2 in. in 2 hr, 4 in. in a day.



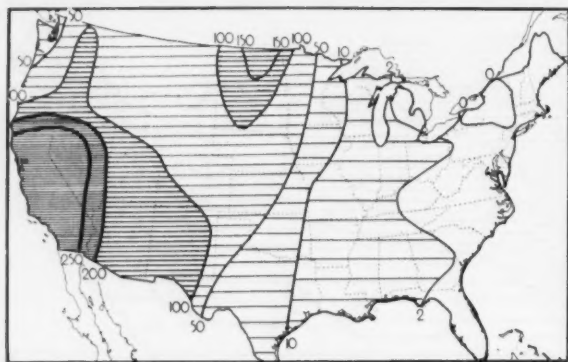


Fig. 12. Number of months in 50 years (600 mo) that had less than 0.5 in. of precipitation, based on state averages.

only the southern half of Florida and the southern twelfth of Texas have fewer than 1000 "cold degree-day units" a year, whereas only sparsely populated areas of the North and West have more than 8000 such units. These units are the sums of the average daily temperatures below 65°. A day with an average outdoor temperature of 40° contributes 25 such units to the total. In the densely peopled parts of the country, the average number of cold degree-day units is mostly between 4500 and 6500; for example, it is 4500 in Washington, D.C., 5400 in New York, N.Y., 5900 in Boston, 6800 in Buffalo, 6200 in Cleveland, 6300 in Chicago, 4600 in St. Louis, 8000 in Minneapolis, 4900 in Seattle, 3100 in San Francisco, and 1400 in Los Angeles. Philadelphia has more than twice the units that Birmingham or Atlanta has, Milwaukee has one-eighth more than Chicago, and Cleveland a fourth more than Cincinnati. These variations do not mean quite so great a difference in the fuel supplies needed, however, because of differences in the insulation of houses, for example; but they do reveal an important contrast in an appreciable item of the cost of living.

The fluctuation from year to year in the heating and cooling requirements, as revealed by the number of cold and hot degree-day units, is much less in the northeastern quarter of the country, from Kansas to New York, than in the South or in southern California. For example, in the Southeast (North Carolina to Florida.), the fluctuation from year to year of cold degree-day units is several times as great as in the North. Southern California also frequently has winters that are much colder than normal and, hence, is then distinctly less attractive to winter tourists.

The difference in the temperature at the hottest and coolest time of the day is also significant. In midwinter, the range is about 20° in most of the

country, but it is more than 26° in the arid southwest and less than 18° in four areas: the far northwest, near the Great Lakes, and on the coast from Maine to Maryland, and also on the Gulf of Mexico coast west of Louisiana. In midsummer, the range is somewhat less than 20° from northern Texas to Florida and north to Cape Cod, least (less than 15°) on the Texas coast. For the western half of the country, it is more than 25° except on the Pacific Coast, and more than 35° in a large western interior arid region.

Everyone knows that it often feels hotter or colder than the thermometer reports. The "sensible temperature" varies with the wind, sunshine, and atmospheric humidity. When there is little evaporation because the air is humid and stagnant, high temperatures are especially oppressive. There are striking contrasts in average July temperatures of thermometers enclosed in a wet cloth because such a thermometer reflects the amount of evaporation. The Gulf Coast, nearly all of Florida, and the coast of South Carolina have July wet-bulb temperature averages of 75° or higher, most of the rest of the South from 70° to 75°, and a belt from southern Maine to southern Minnesota and thence south to western Texas has 65° to 70°. The rest of the North and the West have July wet-bulb temperatures of less than 65°, much of the West from 50° to 55°. Thus, in the desert, high temperatures in the shade feel much less hot than do distinctly lower temperatures in the humid South.

Precipitation contrasts. Approximately the eastern half of the United States normally receives sufficient rainfall to be classified as humid; most of the western half is semiarid or arid. The humid region receives an average of 20 in. on its drier margin, up to more than 60 in. in parts of the Southeast. The state with the highest average precipitation is Louisiana, with 55 in.; the driest one is Nevada, with 8.8 in. For parts of states, the wettest areas in the East are the southern end of the Appalachian Mountains and along the eastern Gulf Coast, with averages of somewhat more than 65 in. In the arid West, the precipitation averages less than 2 in. in Death Valley and in the Imperial Valley of California. At the other extreme, it exceeds 80 in. in extreme northwestern California and in western Oregon and Washington, where the national record of 150 in. is held. The semiarid region receives an average of 10 to 20 in., with wide annual variation. Indeed, that region is "sometimes arid, sometimes humid, rather than halfway between arid and humid." The annual percentage fluctuation in arid regions is even greater than in semiarid areas—normally desert

Fig. 13. Evaporation regions:
1, evaporation much in excess
of precipitation; 2, evaporation
considerably in excess of pre-
cipitation; 3, evaporation usu-
ally in excess of precipitation;
4, evaporation in excess of pre-
cipitation in warm months; 5,
precipitation in excess of evap-
oration in cooler months; 6,
precipitation generally in excess
of evaporation.

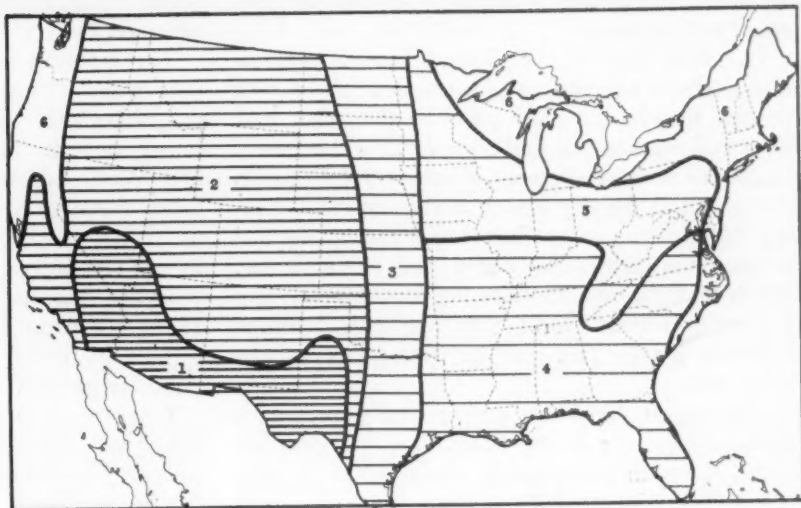


Fig. 14. Average surface wind
velocity at 3 P.M. local time,
normally the hour of most wind.

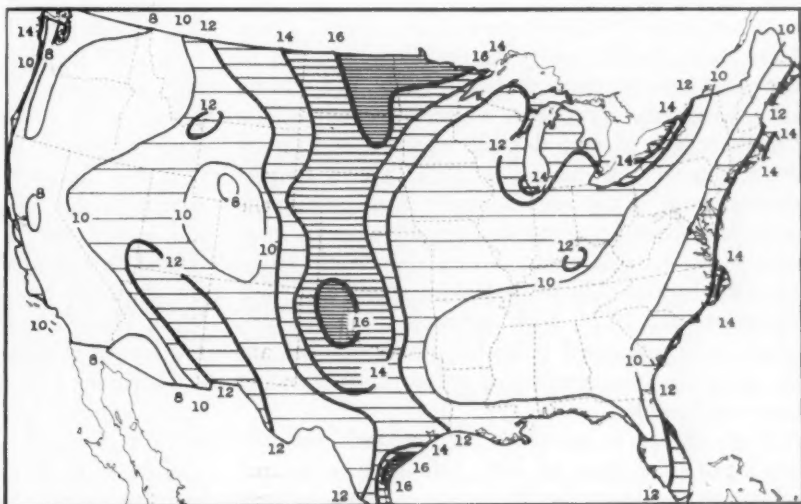
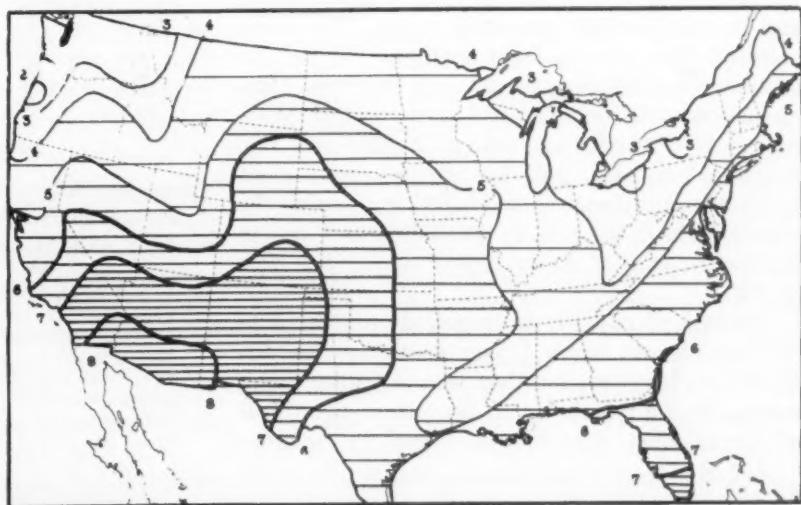


Fig. 15. Normal daily number
of hours of sunshine in winter.



areas are sometimes brilliant with flowers and green with grass.

In about half of the United States, approximately equal amounts of precipitation fall in each normal month, but four parts in the other half have well-marked seasonal contrasts. On the Pacific Coast, the winters are rainy, the summers dry, with the length of the dry season increasing from western Washington, where it is 2 to 3 mo, southward to southern California, where it is 8 to 10 mo. Much of California receives no rain at all during 7 summer weeks. Florida's tourist trade is much benefited by the fact that its winters are usually fairly dry.

The largest American region with conspicuous seasonal contrasts in the amount of precipitation is the one with little winter precipitation. It centers in Nebraska and extends from northern Texas to Minnesota and central Montana. It normally has considerable rain in the late spring and early summer but little rain or snow in the winter. These facts favor stock raising, partly because much of the grass usually cures to natural hay and seldom is snow-covered too deeply to permit grazing. The fourth region of considerable seasonal contrast is New Mexico and eastern Arizona. There, despite dryness most of the year, summer rains permit the growth of corn far more successfully than in most of the West. This fact favored the development of early Indian cultures based on corn. The Cliff Dwellers and Hopi, with permanent settlements, were more advanced culturally than the Indians of other dry western areas, where corn growing was not feasible.

With regard to snowfall, the regional contrasts are great, less than an inch falling in a normal winter in the Deep South and in extreme western California, and more than 100 in. in western Maine, western New York, northern Michigan, and in many western mountain ranges. The 10-in. snowfall line extends from southern Virginia to southern New Mexico, the 50-inch line from Massachusetts to Minnesota, and from central Montana to New Mexico and eastern Washington. The Cascade Mountains and the Sierras receive great totals. One California record is 884 in. (during the winter of 1906-07); 60 in. has fallen in 1 day in California, and 42 in. in 2 days in western New York.

"Excessive" rainfalls are especially numerous in the Deep South. In the Upper South they are distinctly less frequent, and they are rare in the cooler season. In the remainder of the eastern two-thirds of the country, they are occasional in summer,

while in the West, except on the rainy coast, they are rare.

The amounts of rain that fall in short periods vary widely. In thunderstorms, wherever they occur, short hard rains are normal. But falls of 10 in. or more in 24 hr are very rare in the North and West, except locally in California, but are fairly frequent from Texas to Florida, each of which has several records of somewhat more than 20 in. The highest Weather Bureau record is held near Los Angeles in the mountains, with 25 in. in 24 hr, but some dependable non-Weather Bureau records in Texas and even in Virginia are considerably greater.

Sunshine contrast. With regard to sunshine, there are conspicuous regional contrasts in the amount and season. For the year as a whole, the Pacific Northwest and a part of the Northeast receive an average of less than 2200 hr of sunshine, or about 6 hr per day. Southern Florida and a large southwestern region receive, however, more than 2900 hr, or 8 hr per average day. A considerable southwestern region receives 10 hr and is sunny much of the year. The number of cloudy days per year is 100 to 130 in most of the East, but it is less than 60 in a large southwestern region. Although the Southeast, especially southern Florida, is well ahead in winter in sunshine, it is surpassed in summer by much of the North, where the days are longer, as well as by the more arid West. Florida and the rest of the Deep South get much rain in summer, and with it, cloudiness.

Summary. Climatically, the United States affords much variety. Indeed, equivalents of most of the world's climates can be found somewhere in the United States during at least part of the year. This helps to prepare qualified Americans to succeed in other parts of the world. Clearly the variety of climate contributes in diverse ways to the strength of America. The wide departures from average that are experienced almost everywhere at least stimulate human adjustments or adaptability.

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Concerning the Nature and Nurture of Genius

SIDNEY L. PRESSEY

Dr. Pressey has for some 30 years been professor of psychology at Ohio State University where he has published extensively and directed numerous investigations in educational, social, and developmental psychology. His graduate training was at Harvard. In the last dozen years he has given major attention, under special subventions, to problems of educational adjustment in the war and post-war periods with special reference to the superior student.

IT has been well said that "in the present international tug of war, survival itself may depend upon making the most effective use of the nation's intellectual resources" (1, p. 4). Means of better identifying young people of superior intellectual capacities, and of getting more of them into present programs of advanced training, have been widely discussed. However, there has been relatively little consideration of whether present educational programs are best suited to the needs of our most brilliant young people. Superior abilities are now generally considered so predominantly a product of innate constitution that certain "educational" factors, possibly of very great importance in the growth of such abilities, are overlooked. It is sometimes well to get outside of current habits of thought and try to look at a topic in a reappraising way. This paper attempts so to do. It focuses attention on that most extraordinary type of very superior intellect—the precocious genius—as possibly exhibiting especially clearly both innate capacities and developmental influences involved in extraordinary accomplishment. It presumes to suggest that there may be ways by which many more "geniuses" might be not only discovered but even, to a substantial degree, made and brought to fruition.

Major Factors Making for Precocious Marked Superiority

An informal search for instances of marked precocity suggests that such cases have been especially frequent (or, at least, especially noted and featured) in certain fields and in certain localities at certain times. In the Europe of 100 to 200 years ago there were outstanding musicians of whom most were precocious. Handel played on the clavichord "when but an infant" and was composing by the age of 11. Haydn "played and composed at the age of 6." Mozart played the harpsichord at 3, was composing at 4, and was on a tour at 6. Chopin

played in public at the age of 8; Liszt, at 9; Verdi, at 10; Schubert, at 12; and Rossini, at 14. Mendelssohn was playing publicly and also composing by the age of 9, as was Debussy at 11, Dvorak at 12, and Berlioz at 14. Wagner conducted one of his own compositions in public when he was 17 (2, 3, 4).

Recently, and especially in this country, there have been many precocious athletes. Thus, Bobby Jones was state golf champion at 14 (5). Marlene Bauer was playing notable golf at 13 (6). Sonja Henie was figure-skating champion of Norway at 10 and world champion at 15. Barbara Anne Scott was Canadian figure-skating champion at 15 (7). Vincent Richards was national tennis singles champion at 15, and Maureen Connolly was woman's singles champion at 16. Mell Ott was in big league baseball at 16; Feller, at 18. Eddie Lebaron was an intercollegiate football star at 16. Bob Mathias won an Olympic gold medal in track and field events at 17 (5). The reader may doubt whether athletic champions are relevant to the topic of this paper but he can hardly question that they are very competent in their fields. Moreover, precocity in athletics and in musical performance might seem especially extraordinary because finger reach and dexterity on a musical instrument and strength, as well as skill and endurance, in athletic competition would seem especially to call for physical maturity.

An especially remarkable type of athlete must also be noted: the champion whose superiority emerged after great and persistent efforts to overcome a crippling handicap. Walt Davis, holder of a world's record in the high jump, was a former polio victim (8). Glenn Cunningham, the Olympic runner, was burned so severely in the legs when he was 8 years old that it was doubted that he would ever walk again. At the age of 8, Nancy Merki had polio and at 10 was more paralyzed, but at 13 she was high-point scorer in a national swimming meet (5). Other well-known athletes have had polio. In

all these instances, the athletic prowess was the final result of very persistent (and usually expertly guided) efforts to overcome the handicap. There was no evidence before the injury or illness of notable athletic potential.

The first question, then, is why there should have been a rash of notable precocious musicians in the Europe of a century and more ago and a spate of youthful athletic champions in this country now. Certain major factors seem evident. In the Europe of that time, music was the major popular interest, reaching practically all social classes and all ages, and offering even to underprivileged youngsters the possibility of wide popular acclaim. Athletics is a similar interest in this country now. The second question is more specifically how, in such favorable total situations, have these prodigies come about. A study of their careers suggests that the following factors are important.

1) Precocious musicians and athletes usually had excellent early opportunities for the ability to develop and encouragement from family and friends. Mozart's father was a musician; his older sister was his companion in music; family and friends admired and encouraged the boy. Schubert's father was musical and fostered Franz's musical aptitude; soon Franz became a member of a string quartet with his father and two brothers. Of the athletes, Bobby Jones lived next to a golf course. When he was still a little boy, he was given small clubs and followed his father around the links. From early childhood, Barbara Anne Scott's skating was fostered by her father, and soon her whole life was so centered.

2) Usually individuals who developed precocious excellence had superior early and continuing individual guidance and instruction. From the age of 3, Mozart was taught, guided, and managed in his career by his father, who sought practically from the beginning to make his son an outstanding musician. Mendelssohn, also from the age of 3, was taught by his mother and other good musicians. Marlene Bauer's father, a golf professional, began to teach her the game when she was 3. Nancy Merki had an expert swimming coach.

3) Precocious individuals have had the opportunity frequently and continuingly to practice and extend their special ability and to progress as they were able. From the age of 3, Mozart practiced with his older sister; he had the opportunity to play the violin, the harpsichord, and the organ, to perform frequently in public, and a little later to conduct. From the age of 11, Maureen Connolly practiced tennis at least 3 hours a day. The climate of southern California made this possible at all seasons. She took on more able opponents and en-

tered more important tournaments, as she was able. Nancy Merki was "in the water for hours at a time, just trying to master the trick of fluttering her legs." Under the guidance of her coach she moved forward in her aquatic accomplishments as she was ready.

4) The special precocious ability usually brought a close association with others in the field, which greatly fostered the abilities of all concerned, and led to a still wider stimulating acquaintance. Mozart lived from early childhood in a world of musicians who listened to and watched one another, played together, cooperated, competed, raised levels of aspiration, and were keen in criticism and encouragement. His musicianship brought acquaintance with the great all over Europe, including the Austrian emperor. Bobby Jones lived largely in a golfers' world, which developed his skills at the same time that he raised golfing standards and increased the popularity of the sport. His friendships have indeed been wide, including President Eisenhower.

5) As a result of many opportunities for real accomplishment, within his possibilities but of increasing challenge, the precocious musician or athlete has had the stimulation of many and increasingly strong success experiences—and his world acclaimed these successes. It is well recognized that frequent failure and continued frustration may debilitate personality and competency, just as a disease does. But the opposite also seems true, although it is not generally appreciated: frequent, much-admired successes increase effort, build up psychosomatic vigor, make attempts more vigorous, and adequate, and better integrated, and build ability. The opinion is ventured that such "furtherance" is as important a phenomenon as frustration, and that systematic research regarding furtherance might well be as profitable as research on frustration has been.

At any age, development of any ability is fostered by a favorable immediate environment, expert instruction, frequent and progressive opportunities for the exercise of the ability, social facilitation, and frequent success experiences. Important advantages would seem to accrue from having these factors begin operation early. The physique may grow and adapt in congruence. As the young musician stretches out his hands and exercises needed muscles and coordinations, his skills may be not only learned but somewhat made part of his growth. This might be true, not for mechanical skills alone, but also for related integrations in the central nervous system, and for related percepts and concepts. So the precocious musicians played not only with skill but also with understanding,

and they composed, notably and early. Possibly some integration of learning and growth might occur with abilities less closely related to a skill. Any ability, developing early, might benefit by having the great energies of childhood and youth devoted to it. Also, the child in the grip of a strong interest (as a hobby) seems often single-mindedly absorbed in it to an extent less common later, when problems of social status, economic responsibility, or the other sex may distract. If an interest is already well established when adolescence comes, the energies of that period may pour into it.

The thesis thus is that, in attempting to account for notable precocity in such fields as music and athletics, too much stress has been put on presumed extreme constitutional genius and too little on a concomitance of favorable factors, operating in the growth years. Instances of great athletic skill emerging from efforts to overcome a seemingly crippling handicap seem to emphasize the potency of these last factors. Presumably, the original physical potentials of these individuals were good, although not manifest, before the handicap struck; but the great potential of favoring circumstances seems especially evident. In this connection, Wechsler's argument (9) may well be recalled—that the range of human physical traits, as in height, strength, and quickness, is really not great, and that the range in mental capacities may be less extreme than is usually supposed. Rather, superior original capacity, *growing under a favorable concomitance of circumstances*, develops into genius.

So far, the discussion has dealt primarily with outstanding precocious skills, in athletics and music. May the youthful organism not be capable of outstanding accomplishments more intellectual in nature? Here it should again be mentioned that notable musical performance would seem to involve keen musical understanding as well as dexterity (and outstanding athletic performance perhaps often involves more intelligence than is usually conceded). In youth, the famous precocious musicians not only performed but composed notably; and composing music is surely a highly intellectual activity.

But precocity has appeared in sundry other and clearly intellectual fields. John Stuart Mill began the study of Greek at 3. By the age of 8 he had read Xenophon, Herodotus, and Plato and had begun to study geometry and algebra. At 12 he began logic, reading Aristotle in the original. The next year he began the study of political economy; at 16 he was publishing controversial articles in that field (10). When he was 6 years old, John Ruskin wrote verse. Macaulay compiled a "universal history" at the age of 7. Published poems of

William Blake, Thomas Chatterton, and Alexander Pope go back to their 12th years; poems of Robert Burns go back to his 14th year, and of Milton to his 15th year. Pascal wrote a treatise on acoustics when he was 12. Galileo made his famous observations of the swinging cathedral lamp when he was 17. Perkin discovered the first synthetic dye when he was 18 (11, pp. 198-219). Farnsworth, at 15, "evolved an electronic means of sending pictures through the air" (12). Recently, 11-year-old Italian Severino Guidi, 10-year-old Turkish Hasan Kaptan, and 11-year-old French Thierry Vaubourgoin have been mentioned (13) as precocious painters. Norbert Wiener has written his sensitive account of his own precocity: college entrance at 11, Harvard doctorate at 18 (14). However, as compared with music and athletics (15), precocity seems more rare in art, literature, and science, and especially so in this country. Why?

Influences Hampering the Precocious

There is a general belief, fostered in this country by most child psychologists and "progressive" educators during the past 25 years, that intellectual precocity is somehow not quite healthy, is almost always a hazard to good social adjustment, and should be slowed down rather than facilitated. In the home, the early-reading precocious child causes anxiety, in spite of the usualness of such precocity in Terman's gifted group and in biographies of famous men (16, 17). The schools oppose entrance before the standard age of 6, in spite of the evidence, from some half-dozen experiments, that gifted tots admitted earlier have done well, both academically and adjustment-wise (18). The general public tends to regard the intellectually gifted small child as a freak. In short, there is usually none of the initial encouragement in the family, early fostering, and favorable general social climate that got many musical and athletic prodigies off to a flying start.

As a result of mass education and indifference to the needs of the gifted, there is almost none of the individualized guidance and instruction for excellence that was mentioned as an important element in the rapid development of precocity in music and athletics. A good music teacher is usually especially interested in finding and training pupils who are gifted musically. The athletic coach tries to find and bring to peak performance the ablest young athletes in his school. But the usual public school teacher does not have the time, the attitude, or the methods to do much, if anything, for another young Macaulay or Farnsworth in his classes.

In contrast to possibilities of continuing intensive practice and rapid progress in music or athlet-

ics, opportunities often are entirely lacking for a youngster to indulge intensively and continuingly an aptitude in such a field as a science, advancing as he is capable. A boy precociously interested in chemistry may have to await schoolwork in that subject until the regular course in his high-school junior year. He must then start and progress with his classmates, and in his senior year must take other subjects (intensive work in one field is frowned upon as interfering with a broad program) and not "hang around" the chemistry laboratory (19). Nor can the broadly gifted and precocious youngster advance in his total school program more rapidly than the average; acceleration is, in most schools, considered unwise.

Whereas the precocious young musician or athlete soon has an acceptance and a mounting status that is tremendously stimulating and educative for him, in musical or athletic groups—and these groups have status in school and community—the budding young scientist or scholar may be isolated or may associate only with a friend who is also considered "odd" or may belong only to an anemic subject club of no prestige in the community.

In contrast to the early and continuing successes of the young athlete or musician, possibly mounting to international acclaim, the young scholar or scientist may have no opportunities to make good except in class assignments and may obtain no evidence of success other than good marks. The teacher (perhaps made uncomfortable by keen questions) may even criticize his intense interest, and the other youngsters may call him sissy or odd. For him there is frustration, *not* the furtherance of cumulative success.

Suppose that Mozart or Bobby Jones had not been allowed to begin his music or his golf until the other children did, or to practice more or progress faster, or had had only the instruction of a school class in music or physical education. Suppose that they had been kept from playing with older children or adults in the fear that they might become socially maladjusted, kept from associating much with other musicians or golfers because that would be narrowing and undemocratic, kept from public performances or tournaments because that would be exploiting the poor child! It surely may be questioned whether they would then have reached the preeminence they did. Abuses in the afore-mentioned directions are, of course, possible. But it is also an abuse to withhold opportunities from precocious youngsters who are eager to advance and excel. The opinion is ventured that the last type of abuse is now, in this country, the more common one.

Toward More and Better American Geniuses

The hypothesis thus is that a practicing genius is produced by giving a precocious able youngster early encouragement, intensive instruction, continuing opportunity as he advances, a congruent stimulating social life, and cumulative success experiences. In the instances given however, the circumstances have all been so superior as to seem somewhat out of reach. Moreover, there was sometimes imbalance or exploitation. In the average college or school, what steps might be possible that would move with reasonable caution and good sense in the directions indicated here and perhaps somewhat benefit a great many youngsters as well as occasionally help toward the production of a "genius"? Two steps would seem feasible and of great possible fruitfulness.

The first proposal is that there should be, in a college or a school system, a person who might be given the somewhat colorless title of coordinator of special programs, lest the more precise label of personnel specialist for superior students cause them embarrassment and antagonize parents of students not selected or served by him. Such a person in a college should scan each entering student's record to find high-school valedictorians, science-fair winners, and others with evidence of superior ability. He should watch for such evidence especially among students in the freshman year. He might even follow reports on high-school science fairs and the like and recruit promising youngsters for his college in the manner of a football coach. (If other colleges object to this, maybe competition among colleges for the intellectually superior might be a good thing!) As he locates such cases he should seek them out, encourage them, and bring them to the attention of appropriate faculty members. He should try to help these students in any problems they have, find opportunities for them on campus, and perhaps arrange summer work or travel opportunities. He should make a special effort to bring congenial members of his group together and to foster stimulating companionship and morale. He should see to it that his program receives publicity and that his youngsters receive recognition. He should guide and further any plans they have for professional or graduate training and for careers.

In a secondary school or school superintendent's office, a person similarly designated to find and foster the most able students would try to keep the elementary schools alert to discover especially bright children there. As these move on to high school, he could watch for them. He would have the high-school teachers inform him of outstanding students in their classes and keep alert for other evidences of talent, as in hobbies. He would become

acquainted with all such youngsters, encourage them, and bring them into contact with appropriate teachers and into appropriate subject clubs or other groups. Educative trips with other youngsters might be arranged and perhaps summer work that would be both financially and educationally profitable. A local business or professional man might be enlisted to sponsor an outstanding youngster who needed such support. Contacts might be readied with a college or university.

If such guidance or personnel specialists for the most able were generally available in colleges and high schools or public school systems, it is believed that they could greatly increase the number of young people going into advanced training, select them better, and greatly improve the effectiveness of their education (20, 21). Such a position might be only half time, for a student counselor or assistant principal, but it should be seen as his distinctive opportunity. If in a college, he would work with the ablest students, the best teachers on the faculty, and the best professional and graduate schools. If in a secondary school, he would deal with the finest students and the community leaders most interested in young people. He would try cumulatively to build community interest in and opportunities for these ablest young people, as through the local papers and service clubs. He would have mutually profitable relationships with the best colleges and universities. At regional and national meetings, as of guidance associations, these personnel workers at all levels would meet with others doing like work. Slowly, they might change public attitudes to interest in the intellectually, as well as the athletically, able. Surely no position could be more finely rewarding.

It is not enough, however, to provide special student personnel or guidance service for superior students. *In proportion as they are very able and especially as they have special talents, special adaptations of the usual curriculums are likely to be desirable.* The able youngster not yet sure of his special interests may wish to explore very widely. Once he has found that interest he may, legitimately, wish to push it hard. Before long, his accomplishments may warrant his admission into courses ahead of his status. (The sophomore may desire some course not usually available before the junior year.) Soon, he may be ready for an independent project under supervision of one of the ablest teachers, for an honors seminar, perhaps a project off campus or work experience in the field of his interest—first attempts at real accomplishment in that field. There should be readily usable administrative machinery—it might be called an honors program—making it possible for an able

student, perhaps under the guidance of a person as mentioned in the previous paragraphs, and under the general direction of a faculty committee, to have certain curricular freedoms and special opportunities to foster best his potentialities (22, 23).

It should be possible to adapt school and college programs to the needs of superior youngsters with regard to not only the nature of these programs but also their length. Occasionally a late start or an added year in school or college may be warranted. Far more often, an early start and rapid progress are desirable. Not only the occasional prodigy but most people of superior abilities show their superiority early and develop more rapidly than the average person (17, 24). Moreover, impressive evidence indicates that intellectual creativity reaches its peak relatively early in adult life (11). The practically universal American educational policy, nevertheless, is the lockstep: every child must enter school at 6 (none more than a month or so earlier), progress a grade a year, and, if he seeks advanced training, continue his schooling often till around 30, which was the median age of receiving the doctorate in this country just before World War II. Now, military service may delay even more the completion of education. Yet numerous studies are practically unanimous in showing that able children can enter earlier and progress more rapidly than the average child, without harm and often with gain in regard to realized abilities and social adjustment (18, 24, 25). Outcomes have been thus favorable in spite of most common use of the *worst* methods for "acceleration"—grade-skipping in school and a lengthened year in college. Better methods—admission to the first grade on the basis of readiness for school rather than chronological age, replacement of the first three grades by a "primary pool" out of which children would move early or late depending on when they finish primary work, rapid-progress sections doing 3 years' work in 2 in junior and senior high school, and credit by examination in college—should permit each youngster to move through educational programs at his own pace, without being conspicuous if his rate is not that of the average.

Not only are accelerates usually successful and happy in school, but they are more likely to complete collegiate and advanced training. At Ohio State University, 50 percent of the students entering when they were 16 years old graduated, but only 38 percent of the 18-year-old entrants paired with them according to tested general ability at entrance and type of program. With selection for acceleration and guidance therein, outcomes should be even better. Of a group of students selected in their freshman year as capable of finishing a 4-year

program in less time and guided in so doing 63 percent graduated. Further, accelerates seem more often successful in their careers than individuals proceeding through their education at the usual pace. From 1880 to 1900, 29 percent of those graduating from Amherst at 19 became nationally known, as compared with 12 percent of those graduating at 22. (18.) Of those in Terman's gifted group who graduated from high school under the age of 15 years and 6 months, 16 percent more graduated from college and 19 percent more took one or more years of graduate work than did those who finished high school when they were 16 years and 6 months or older, although there was little difference between the two groups in general ability when they were tested in childhood. (The average IQ's of the two groups at that time were 158 and 149, respectively). Moreover, twice as many of the first group (42 percent as compared with 19 percent) were very superior in respect to career (17, pp. 265-279).

In short, simply to increase the number of bright American youngsters who "accelerate" should substantially increase the number obtaining technologic or other advanced training and make it easier for precocious genius to emerge. If it were possible for bright youngsters not only to move through school more rapidly but also in other ways to have their programs adjusted to their special needs, still more might be expected to complete such training, still more successfully, and with still more notable careers following. Moreover, they would finish their training and get into their productive careers sooner. And educational costs would probably be reduced! Thus it seems a reasonable estimate that every year there remain in the secondary schools around 300,000 students whom a reasonable program of acceleration would have graduated. Such a reduction in enrollment would involve substantial savings, which might more than provide for the suggested special counselors for the gifted.

To meet the needs for trained manpower mentioned at the beginning of this paper, greater efforts to interest bright students in collegiate and advanced training programs (as they are now), better guidance of students in those programs, and more scholarship or other financial aid, have been suggested. The suggestion is here ventured that special facilitated programs adapted to the needs of the gifted would be the best means of interesting them, that special guidance in such programs (as suggested here) would best keep these students in school, and that such facilitated and early-completed programs (often including paid work experience) would substantially reduce the need for financial aid to students. Finally, the proposed

special measures should produce more "geniuses." To produce persons of notable accomplishment, educational efforts should be directed straight toward that goal, in the light of all that can be found out about such persons and their upbringing. Simply to increase the number of students in physical education classes would probably not very much increase the number of athletic champions!

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16. C. M. Cox, *The Early Mental Traits of 300 Geniuses* (Stanford Univ. Press, Stanford, Calif., 1926).
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18. S. L. Pressey, *Educational Acceleration: Appraisals and Basic Problems* (Ohio State Univ., Columbus, 1949), p. 11.
19. Subjects that might arouse early interest often cannot be started in high school. Thus, a youngster may have no opportunity to study psychology until his junior year of college. But trial of group discussion of social adjustment in junior-high-school and secondary-school home economics courses on "family life" have demonstrated the possibility of early beginning of a colloquial psychology. Children may be very shrewd in human relations. The opinion is ventured that precocity in psychology should be especially feasible. Such a youngster, entering adolescence with extensive relevant knowledge, might make distinctive contributions to knowledge of that period.
20. Wolfle (1, p. 251) has reported a study showing a substantial increase in the number of high-school students going to college as a result of a general school guidance program. A recent account (21) dramatizes possibilities of finding and furthering talent in even a small and isolated school.
21. F. V. Rummell and G. M. Johnson, "Bill Lane's students win the prizes," *Reader's Digest*, Jan. 1955, p. 29.
22. A program of this general type was for some years in effect at Ohio State University, but unfortunately it was dropped at the beginning of World War II, primarily because of lack of any such personnel spe-

cial, as has been urged in the preceding paragraphs. Many of the undergraduates in this program did research or service projects worthy of publication (23). The conventional honors program lacks the opportunities for work experience and for the research or service project (as distinct from a paper or library reading) that were found most distinctively

23. S. L. Pressey, "The new program for the degree with distinction at Ohio State University," *School and Society* 36, 280 (1932).
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What shall I wish for the young students of my country? First of all, sequence, consequence and again consequence. In gaining knowledge you must accustom yourself to the strictest sequence. You must be familiar with the very groundwork of science before you try to climb the heights. Never start on the "next" before you have mastered the "previous." Do not try to conceal the shortcomings of your knowledge by guesses and hypotheses. Accustom yourself to the roughest and simplest scientific tools. Perfect as the wing of a bird may be, it will never enable the bird to fly if unsupported by the air. Facts are the air of science. Without them the man of science can never rise. Without them your theories are vain surmises. But while you are studying, observing, experimenting, do not remain content with the surface of things. Do not become a mere recorder of facts, but try to penetrate the mystery of their origin. Seek obstinately for the laws that govern them. And then—modesty. Never think you know it all. Though others may flatter you, retain the courage to say, "I am ignorant." Never be proud. And lastly, science must be your passion. Remember that science claims a man's whole life. Had he two lives they would not suffice. Science demands an undivided allegiance from its followers. In your work and in your research there must always be passion.

—IVAN PAVLOV.

Fortress City of Constantine, Algeria

BENJAMIN E. THOMAS

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AMONG the cities of North Africa, Constantine is unique. Cairo, Algiers, and Casablanca are better known, but Constantine has a site that has been used continuously through all periods from pre-Roman times to the present. Constantine has never ceased, throughout all its history, to be both an important market and the capital of a kingdom, a confederation, or a province. Its long history makes it a competitor of Constantinople (or Istanbul), which is often recognized as the oldest and most often besieged city of Europe. In two other respects Constantine and Constantinople are also similar. Both were renamed for the Roman emperor Constantine—the city of Constantine in A.D. 311, and Constantinople in A.D. 330. Also, both have distinctive sites with strong natural defenses. Constantinople is on a peninsula that can be protected by a wall on the landward side, and Constantine is on a high flat rock with precipices on almost all sides.

The origins of Constantine extend back into prehistory, but by the time of the Roman Empire the site was occupied by the thriving town of Cirta, a trade center and capital of the province of Numidia (Fig. 1).

The Romans extended wheat production in the area, and Cirta became the crossroads of two major paved Roman roads, one running east and west and ending at Carthage, the other running southward from the Mediterranean Sea to the Sahara frontier (Fig. 2).

Cirta was renamed Constantine. The Roman Empire declined and disappeared. The famous Roman roads, after several centuries, fell into disrepair and were covered by alluvial debris or wind-blown material in most places. The successors to the Romans—the Berbers, Vandals, Arabs, and Turks—used only trails for pack animals and pedestrians. They had neither wheeled vehicles nor an administrative system for the use and repair of highways. After a time, even the location of the abandoned and obscured roads was forgotten.

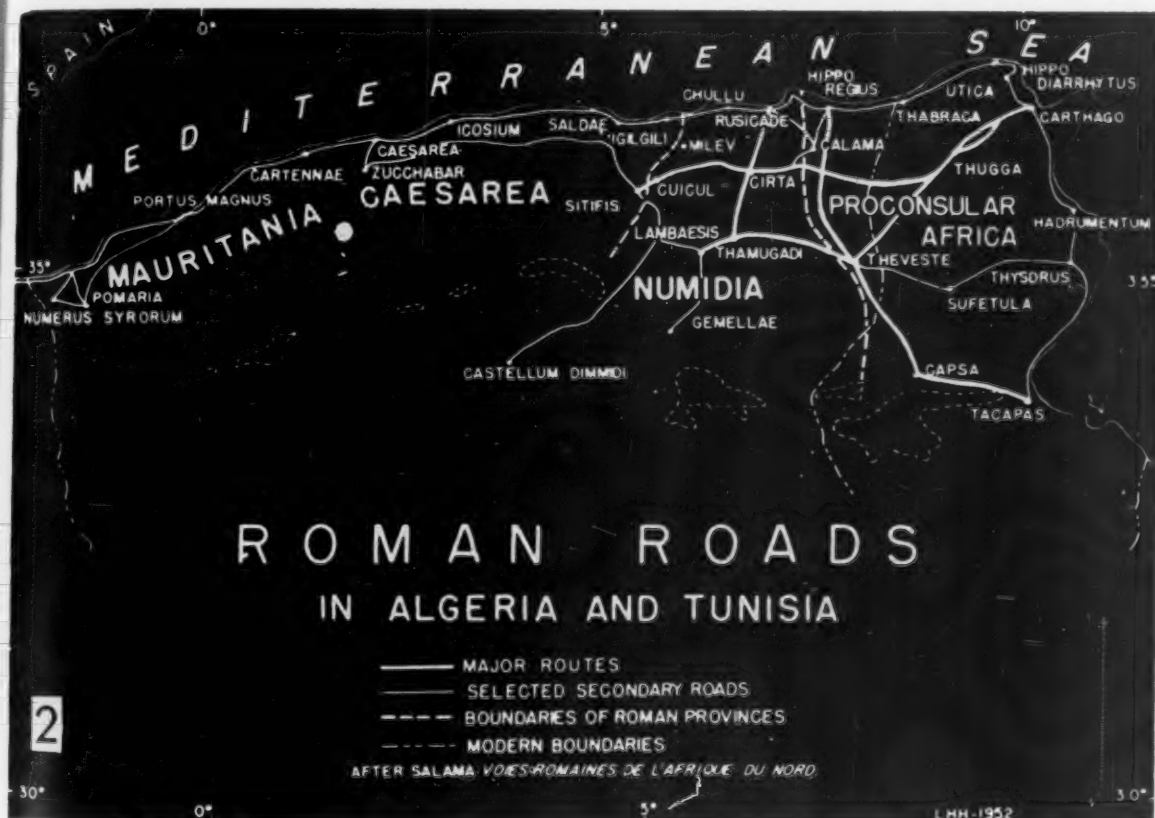
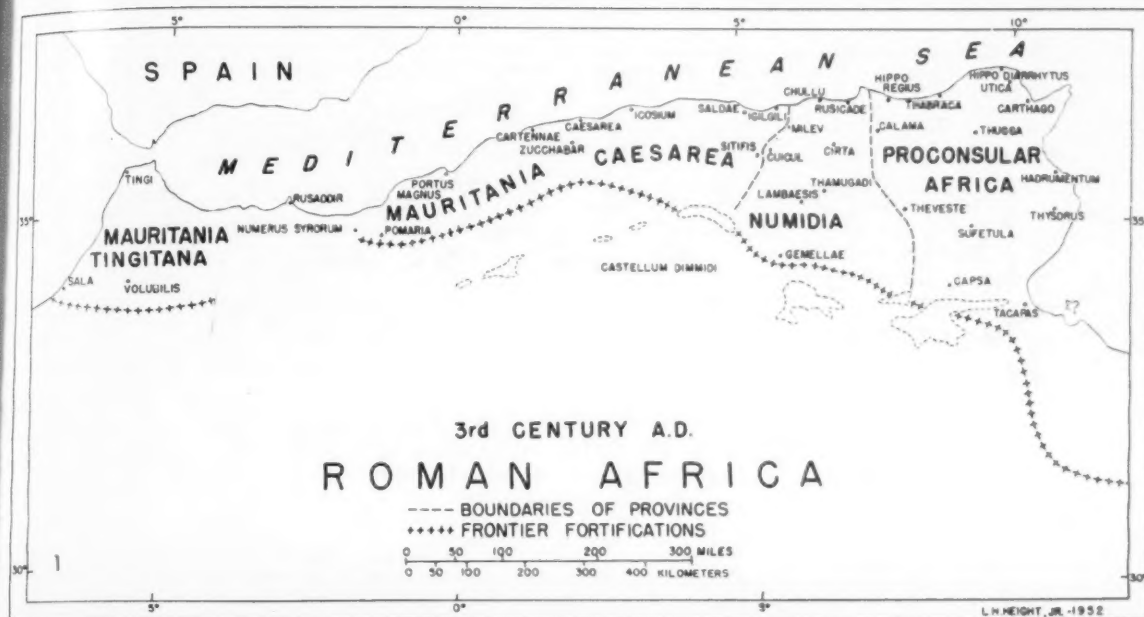
Constantine, however, continued as the main

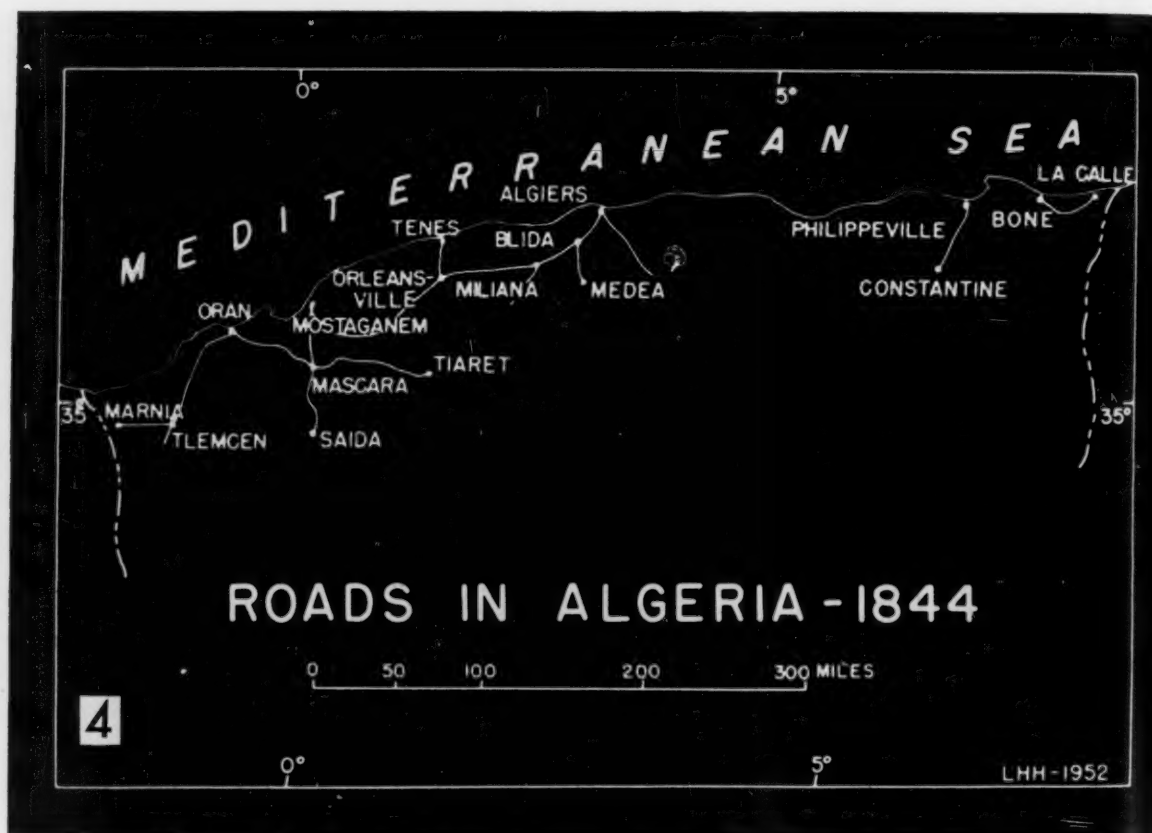
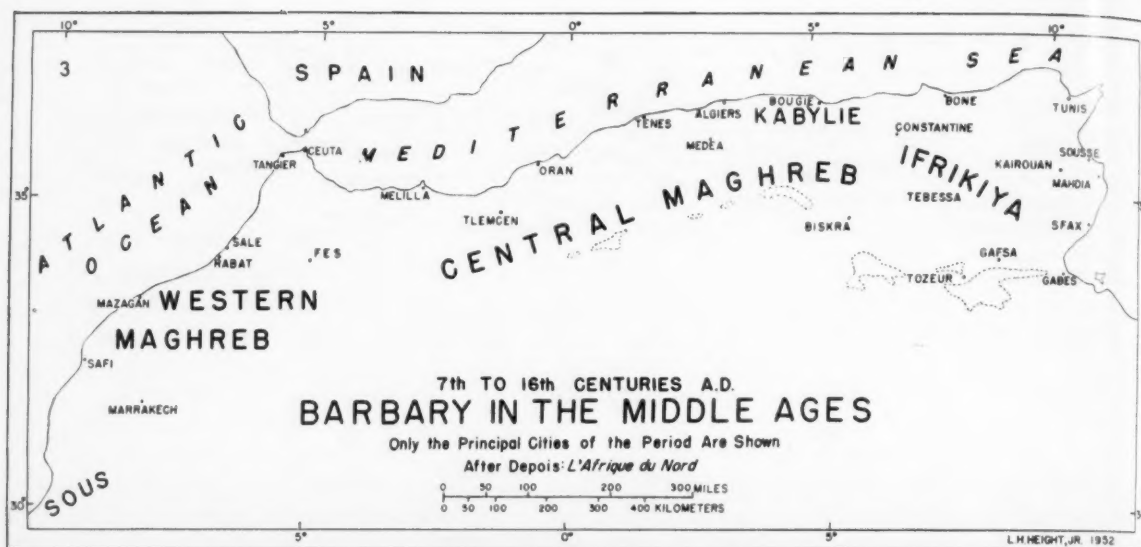
economic and political center of its region (Fig. 3). As soon as one culture declined (and the city with it) another arose, and Constantine recovered its commercial importance. The area for which it was the political capital expanded and contracted with the changing fortunes of warfare, conquest, and settlement. For some time it was a capital and main center of "Ifrikiya," a term that was later changed to "Africa" and applied to the entire continent.

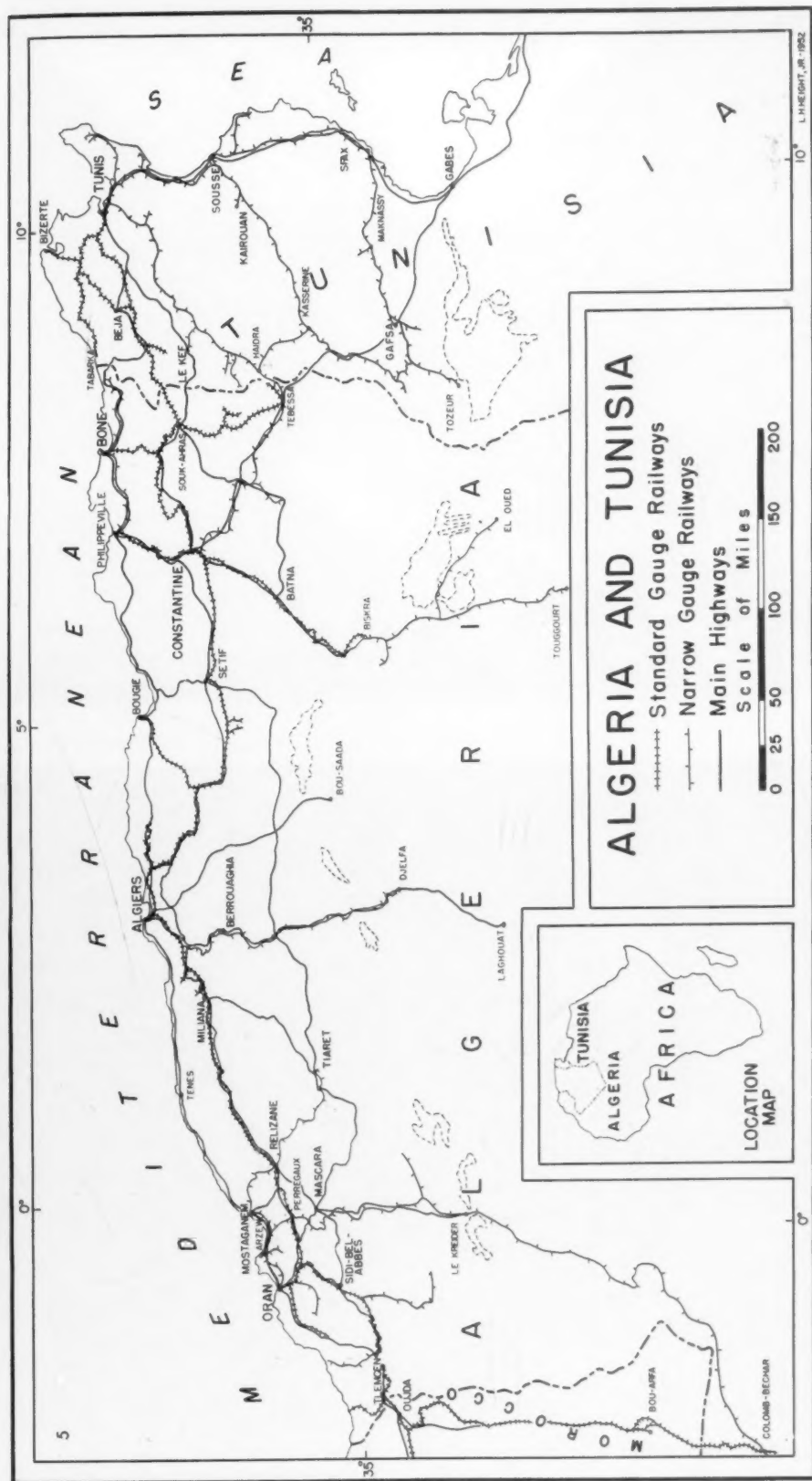
When the French began their conquest of Algeria in the 1830's, the country had no wagon roads—only trails. The Roman roads had been buried for centuries. The French took the seaport of Algiers, for which the country was named, and then attempted to capture the interior fortified city of Constantine. The first attempt was a failure, but, the second siege, although it was difficult and costly, delivered the fortress to the invaders.

Like the Romans before them, the French started a road system to aid in conquest, administration, and trade. Constantine was linked to the coast at an early date (Fig. 4), and after 100 years the French had almost duplicated the Roman road net (Fig. 5). Although the old road beds were not used, the general routes of the ancient highways were followed by both paved roads and railways. Constantine again stands out as the largest city in eastern Algeria, the crossroads of major east-west and north-south routes, and the capital of the Department of Constantine, which corresponds roughly with the Roman Province of Numidia.

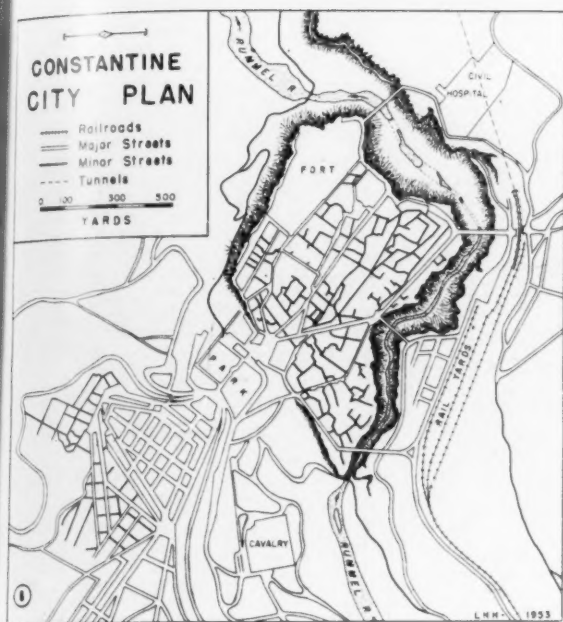
Both the general location, or situation, and the local site of Constantine have contributed to the durability of the city. These have been the stable geographic elements over centuries of changing governments and economies. In a general way the trade area and political unit dominated by Constantine has often consisted of the high plains of eastern Algeria, a farming and a grazing area with mediterranean and steppe climates. Constantine lies near the junction of the two climates and has











been a traditional place for the exchange of farm produce of the north for pastoral products of the south. This primary trade area of good agricultural and grazing land is bordered on the north by the Coastal Atlas Mountains and the Mediterranean Sea, on the east and west by high and rugged mountains, and on the south by the Saharan Atlas and the Sahara itself.

Constantine also serves a larger region; the high plains area is favored as a focus of natural corridors for trade routes. East-west travel across the high plains is easier than through the rough coastal Atlas ranges that parallel the sea, and there are convenient passes that link it to regions on the east and west. Any east-west routes lying farther south are, of course, either in the Sahara or its borders and, therefore, serve comparatively sterile regions. North of Constantine are two important passageways through the mountains that give the city access to the seaports of Phillippeville and Bone. South of Constantine is the gorge of El Kantara (Fig. 6). This is a famous and long-used gateway through the Saharan Atlas to Biskra and other oases of the desert. It is now used by both railway and highway. Constantine's situation, therefore, is on the most fertile plains of eastern Algeria with excellent passageways to the adjoining regions.

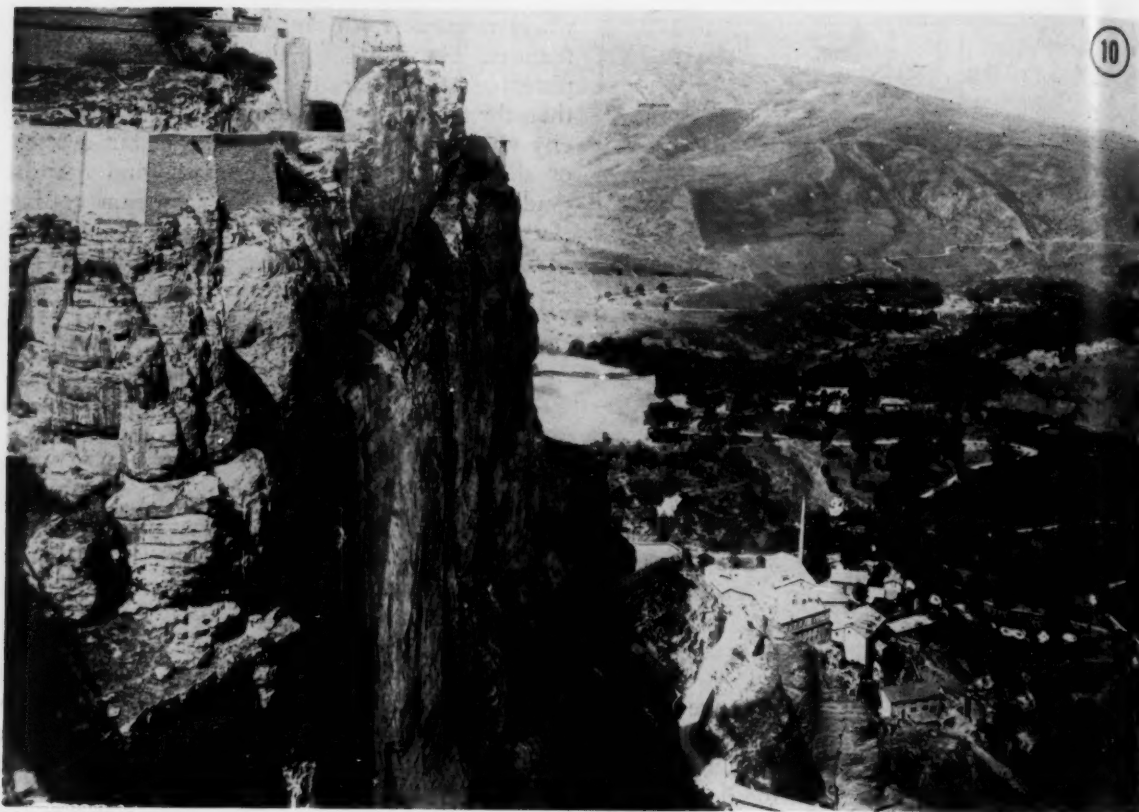
However, perhaps Constantine's site on a flat-topped rock surrounded by canyons and cliffs largely explains the attractiveness of the location (Fig. 7). This site, one of the most spectacular in the world, affords almost ideal protection in a land

where nomadic raids and warfare are traditional features. The city is on the edge of a limestone plateau, which stands about 1000 feet higher than the adjoining plain. Originally the site of the city was joined to the main plateau, but a series of sinkholes, underground streams, and caverns gradually evolved into a continuous stream, cutting the site away from the mainland. Since prehistoric times the Rummel River has flowed between the Rock of Constantine and the plateau (Fig. 8). This leaves Constantine with the canyon on two sides and cliffs on the other two.

In one place (at the right in Fig. 7 and at the south in Fig. 8) the cliff is not steep, thus providing a narrow natural approach to the city. This spot, until the French period, was defended by high massive walls. It was the usual place of attack and has been the scene of numerous pitched battles between invader and defender. In recent years the walls have been removed and the approach has been widened and improved.

The gorge of the Rummel is very deep and steep-sided (Fig. 9). The Romans built the first bridges to join Constantine to the plateau, and the French added others more recently. The western







precipice is equally formidable—a total drop of 700 to 1000 feet with many sheer rock cliffs of dozens of yards (Fig. 10).

In earliest times, nomads met on the rock to trade goods, and the tribal chieftains made it their headquarters. Permanent buildings and walls followed. During historical times the city has been besieged more than 90 times but rarely taken. The natural caves and springs provided enough water at first, but with an increased population it became necessary to maintain underground cisterns of sufficient capacity to withstand long sieges. Constantine now has water piped to the more modern buildings, the same as other cities of Algeria. Because of restricted space, buildings are crowded together, and streets are narrow. In recent decades the French have widened a few streets and cleared a few open spaces, and suburbs of the city have spread to the main plateau.

The seaports of Algiers and Oran are larger, but Constantine is by far the greatest interior city of

Algeria. Since 1900 the population of Constantine has increased from 50,000 to more than 100,000. It is the exchange place for imported textiles, automobiles, machinery, hardware, and other manufactured goods against the local grain, wool, leather, and hides. It is the commercial center for the iron and phosphate mines of eastern Algeria and the capital of a French Department. The European and Jewish population has increased until it almost equals the indigenous Moslem population, and many recent buildings are in modern style (Fig. 11).

There are still sections of old Turkish and Arabic buildings, however, (Fig. 12), and archeological work continues to unearth evidence of the antiquity of the city. Constantine is an outstanding example of a city where a unique site has attracted many peoples of varying cultures to use it as a fortress, trade center, and capital.

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Does Biology Afford a Sufficient Basis for Ethics?

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THE most novel feature of our so-called "atomic age," spiritually viewed, is in all probability the unusual concern that is shown by natural scientists as a group over the dangers of their work, actual and possible. This concern for the moral risks involved in scientific development is evident, among other things, from the new crop of essays that have been published since the eve of World War II by biologists in particular, urging the adoption of a scientific approach to ethical problems. But, however indicative of the seriousness of the present climate of opinion these latest essays may be, at least all of those that have come to my attention suffer from the general defect inherent in the original set of writings on the ethics of evolution that flourished during the last third of the 19th century: namely, they approach problems of value with the same habits of mind and with the same methods of procedure that have proved so successful in dealing with problems of fact (1). I shall try to support this conclusion by examining the most recent and representative attempt, to my knowledge, to put ethics on a biological footing—"Dynamic homeostasis: a unifying principle in organic, social, and ethical evolution," written by a zoologist at the University of Chicago, Alfred E. Emerson (2).

To see how representative this article is, not only of the whole class of evolutionary ethics since the heyday of Darwinism but also of a more general empirical-minded trend in contemporary ethical theory, it would be well to concentrate on its methodological aspects, especially in view of the fact that the author's main "contention" has direct bearing on the crucial problem of ethical methodology (3).

To begin with, I find myself in complete sympathy with Emerson's plea for a position in ethical theory that he refers to as "Naturalistic Ethics." A scientific approach to ethics, by hypothesis, must be naturalistic. So far, so good. However, what is actually defended in his article is, technically speaking, a traditional version of what may be more properly identified as "Positivistic Ethics." By positivistic ethics is meant, essentially, the doctrine that contends that all statements of moral value must be reduced to statements of empirical fact before they can lay any legitimate claim to scientific validity. In a word, the ethics of positivism is thoroughly *descriptive* as against *normative*.

Incidentally, we are deliberately describing Emerson's particular version of positivistic ethics as "traditional" (stemming from Comte in the 19th century), because the up-to-date representatives of the school, armed as they are with a thoroughgoing "verifiability" theory of meaning, have been stressing vociferously for some time the *emotive* significance of ethics as against the *cognitive* significance, thereby making the whole field of morals a branch of rhetoric rather than a branch of science (4). Fortunately, Emerson is wise enough to cling to the classical faith in the possibility of making a science out of our moral judgments. Nevertheless, he spoils this faith by his persistent tendency to reduce the meaning of terms in the moral context to their biological analogs; his entire thesis rests explicitly on the reduction of all the fundamental categories of ethics to Walter B. Cannon's physiological concept of "homeostasis" (5).

The motive behind such a positivistic reduction seems legitimate enough on the surface; a homeo-

static explanation has the apparent "advantage of being subject to objective analysis, quantification, and comparison." Yet, despite Emerson's claim here, the fact remains that to reduce ethical terms to their homeostatic equivalents is to water down their strictly normative connotation, the net result of which is to run counter to that famous insight of John Stuart Mill: "It is better to be a human being dissatisfied than a pig satisfied; better to be Socrates dissatisfied than a fool satisfied" (6). Thus, organic satisfaction, which is homeostasis proper, is not identical with moral satisfaction. A man, like Socrates drinking the hemlock, may undergo organic dissatisfaction and still enjoy moral satisfaction, appearances to the contrary notwithstanding. Such a situation would be impossible to explain on the Emersonian line of reasoning. In brief, any theory that interprets moral conduct in terms other than its own, like the homeostatic interpretation of ethics, rests on the fallacy of reduction. Besides, a proposal to construct a science of ethics is self-defeating if its inevitable outcome is some other science rather than itself.

Given the general positivistic viewpoint that Emerson takes for granted, it should be no surprise to hear that he attempts to bridge the gap between ethical and biological terms by first reducing ethical terms to social ones and then reducing the latter to their biological analogs, such as "the marriage relationship" to "the sexual relationship." I shall leave it to the social scientists to supply the detailed evidence against such a reductive analysis of their subject matter. For our purposes, it is sufficient to note that, from the premise that social phenomena are biologically conditioned, no valid conclusion may be drawn to the effect that they are merely biological in character. The biological foundation of social phenomena has to do with their common genesis, not with their specific characteristics. To assume with Emerson that the genesis of a social phenomenon determines its specific nature is to commit the genetic fallacy. If this argument holds against the reduction of social phenomena to biological phenomena, it holds even more so against the reduction of moral phenomena to biological phenomena.

Moreover, Emerson's positivistic bias comes completely to the fore in the rather impatient way he handles the most comprehensive problem of all philosophy: the relationship of "is" to "ought," which "underlies any science of ethics" and any moral effort of ours. All that he seems to do with that age-old problem is to explain it away with the remark that, in the moral field, "our philosophical difficulties are more semantic than scientifically real." But what, we query, is more "scientifically

real" than the evident gap in our daily experience between the actual and the ideal? All that one has to do to be immediately convinced of this gap is to turn on the radio and hear just a 5-minute summary of the news of the day. No amount of semantic clarification, however impeccable, can legitimately define away the distinction between the actual and the ideal, because it is precisely the function of that clarification to preserve in discourse the very distinction without which no genuine inquiry into ethics is possible. Philosophers, no matter how ingenious they may be in other respects, do not manufacture that distinction out of the blue. Like good empiricists, they first find the gap in our moral experience and then express what they find in the best language they can.

Now, it is this gap in our daily experience that makes the problem of the relationship of "is" to "ought" so exceedingly crucial in philosophy. If Emerson had paid more attention to such a far-reaching problem or if he had been less worried about the controversial character of "philosophical considerations," he would have come out with a stronger case for his timely proposal of a naturalistic approach to ethics. To be sure a naturalistic theory of ethics would maintain that the gap between the actual and the ideal can be lessened without resorting to extranatural or supernatural forces. However, it must be quickly added that moral effort involves not only the possibility of closing the gap between the actual and the ideal to accomplish our purposes but also the possibility of keeping it open to improve them. Thus, the relationship of the real to the ideal must always be kept intact. In order to do this, we must avoid making the two terms of the relationship either so utterly similar or so utterly dissimilar that they would destroy the very condition of moral effort itself—a condition that must permit both the possibility of accomplishment and the possibility of improvement in the sphere of human conduct. At all events, we must never allow our good intentions and earnest zeal for a science of ethics to oversimplify its nature and to underestimate its all-too-real philosophic difficulties. Patience may not always be a virtue in moral life, but it certainly is in moral theory!

Ironically enough, Emerson's impatience with philosophic considerations lands him in a serious difficulty when he comes to his subject of "fluctuating selection pressures in social evolution." To cite his own example: "An individual German could not get a certain job in the late 1930's unless he were a Nazi. But, in the late 1940's, an individual could not get the same job if he had been a Nazi." This is undoubtedly an excellent, although disconcerting, instance of political fluctuations in recent

times, but from a moral standpoint one's reaction to it would be, to put it bluntly, "So what!" Suppose the postwar change in German policy had not occurred; would it not still be true that a Nazi, other things being equal, should never have gotten that job in the first place on the ground that he did not deserve it morally?

I am reasonably sure that Emerson would grant the point here, but the pity is that he does not draw the moral to his own story. Instead, he restricts himself to generalizing that, in social systems, "fluctuating pressures balance each other or result in compromise solutions." With regard to the truth of his social generalization, who can really doubt that compromise is the supreme rule in the arena of politics? Yet, I seriously doubt whether Emerson himself is willing, on that account, to compromise with the devil, morally speaking. If he is not willing to do so—and I feel sure that he is not because of his interest, for one thing, in the all-important issue of reaching scientific agreement on "criteria for the judgment of human conflict," not to mention his explicit concern over the abuse of scientific knowledge for totalitarian purposes—then he should have made it perfectly clear that what holds for politics—"compromise solutions"—does not necessarily hold for ethics. Otherwise he will eventually be forced into the extremely embarrassing position of defending the vicious doctrine that whatever exists has, by the sheer fact of existing, the right to be. Whatever is, of course, *is*, but this does not make it *right*, Alexander Pope's famous line of verse notwithstanding.

The reason that Emerson's commentary on the political fluctuations in Germany since the Nazi regime proves disappointing from a moral standpoint goes back to his fatal mistake of not distinguishing between the two contrasting classes of problems that are to be found on analysis in the social field. These classes of problems are: (i) problems of fact and (ii) problems of value. It should be quite obvious that what differentiates the social field from other fields of inquiry is not that it deals with problems of fact, however complicated and emotionally charged they may be, but that it is confronted with problems of value. Because problems of value are often so difficult to disengage from problems of fact, there is a natural tendency in the hybrid field of the social studies to fuse social problems of value with social problems of fact and, thus, confuse them. Such a confusion between a normative and a factual problem could hardly arise in the field of the physical sciences. At any rate, what characterizes ethics as a *normative* science is that its ultimate concern is with the validity of judgments with regard to what men ought to do, not with the valid-

ity of judgments with regard to what men actually do. And since these two classes of judgments are not necessarily equivalent in meaning, men being what they are, it follows that ethics cannot be reduced to a *descriptive* science without losing its uniqueness as a type of inquiry. Hence, no descriptive science as such, biological or otherwise, can serve as a *sufficient* basis for ethics proper.

All the foregoing remarks have far-reaching consequences for ethical methodology. On the one hand, they imply that Emerson is quite justified in the general and pivotal contention of his essay, that "a partial understanding of value systems is possible through scientific method." On the other hand, they also imply that he is not justified in resting his case on the (positivistic) assumption that science is answerable only to matters of fact and that, therefore, ethics, in order to be amenable to scientific investigation, must adopt the "scientific method and concepts as used in the complex biological sciences." It is precisely this preconception of methodological reductionism, which takes for granted that *only one type* of scientific method is available for all possible subject matters, that vitiates his whole plea for a scientific approach to ethics.

The presence of Emerson's reductionistic preconception in methodology is evident from the very beginning of his article, which starts by putting the cart before the horse, so to speak. It begins with a definition of scientific method, instead of beginning with the recognition and articulation of the problem for which appropriate procedures are to be found and to which they are to be applied. In the particular case under consideration, the problem falls within the field of ethics, but this is beside the point we are making here: namely, that the proper way to initiate any scientific inquiry is to recognize and analyze the problem to be investigated. Why? Because problems determine which procedures are appropriate, not conversely. Hence, ethics, like any other subject matter, can become scientific only by developing a type of procedure (or procedures) that is appropriate to its *peculiar* problems, not by trying to apply artificially a type of procedure that is appropriate to some other class of problems. Our job as methodologists is, or should be, to make the glove fit the hand, not to force the hand to fit the glove. Before we can determine which procedures to use for investigating a designated problem, we must first spot the area of study to which that problem belongs. Otherwise we might be left with a method that is good in general without knowing precisely what it is good for in particular.

Emerson's contention that the scientific method is applicable to the field of ethics is as plausible in appearance as it is hopelessly vague in fact. It is plausible

ble in appearance only because it expresses a truism. After all, to believe in the possibility of a scientific ethics means, simply, to believe in the applicability of scientific method to its subject matter. This is true, of course, by definition. But does this purely analytic assertion imply that scientific method in ethics is identical with scientific method in biology? There is the question!

To be fair, it should be stated that Emerson explicitly acknowledges some difference in the types of scientific procedure within the general field of science. "Because of the complexity of society," he observes, "we may expect to find the scientific methods used in biology applicable to the social sciences rather than those used in the physical sciences." However, he does not realize the full implication of his own comparative observation—an implication that actually contradicts what he advocates. If we cannot expect to find the procedures used in the physical sciences applicable to the social sciences, then by the same token we should not expect to find the procedures used in the biological sciences applicable to the social sciences, let alone to the normative sciences. That is to say, the presumptive evidence against methodological reductionism in one case applies equally to the other. In other words, when a biologist argues for methodological pluralism in comparing the particular methods of his science with, say, those of physics and chemistry, then, unless he has evidence to the contrary, he should abide by the same argument in comparing them with, say, those of economics and ethics. Otherwise, a critic could easily disarm him with the retort that the motive behind his position is more a matter of professional prestige than a question of the naked truth.

The prime question at issue, then, is this: Are the particular techniques (or methods of procedure) that Emerson has in mind applicable to the field of ethics proper? Or, more generally, can a scientific method whose "essential principles" are made in the image of certain natural sciences—the biological ones, to be specific—claim to be exhaustive or representative of the whole field? Hardly! In fact, the greatest irony of Emerson's plea for a scientific ethics is that it rests on a preconceived notion of scientific method. The most profitable way of reinforcing my argument here is to take each of the three "essential principles" that he ascribes to the scientific method and to show the extent to which they are not applicable in their present form to the field of ethics.

It is practically self-evident that Emerson's statement of the first principle of scientific method, "observation by means of sensory perception," is not applicable to the data of ethics, unless we stretch

beyond recognition the denotation of "sensory perception." Moral data as such, in contrast to physical data and, by the way, in contrast also to artistic data (the subject matter of the other normative science, esthetics), are not accessible to *sensory* perception, since they find their expression in those *nonsensory* objects of experience that go to make up the conduct or character of men. Moreover, whereas the difference in complexity between observation in the physical or biological sciences and their counterpart in the social sciences is, admittedly, one of *degree*, the difference in complexity between observation in any of these sciences and their counterpart in ethics is one of *kind*. The difference, for example, between social and moral observation, originates from the two distinct levels of discourse to which they refer. Although observation on the level of social science proper refers to the domain of social *facts*, observation on the level of ethics proper refers ultimately to the domain of social *ideals*.

If this difference in frame of reference cannot and should not be denied, how can a type of observational technique that is appropriate to the domain of social facts (for example, taking a census of the population in a given area) be altogether appropriate to the domain of social ideals (for example, deciding on the desirability of birth control in that area), especially when we know beforehand that its precise function is restricted to gathering the relevant facts about *existing* social conditions, describing those facts as impartially as possible, and classifying the results? Besides, empirical observation would defeat its own purpose if it did not report the facts themselves. In short, no type of observational procedure that is adequate for handling problems of fact, physical or social, can be *automatically* appropriate for handling problems of value. This does not mean that no other type of observation is available for grappling with social problems of value; however, it does point to the irrelevancy of the *sensory* type of observation to the field of ethics.

In spite of the fact that Emerson unduly restricts the observational factor in scientific method to the sensory type of perception, he is aware, like the rest of us, of the *emotional* nature of moral data. He remarks that "ethics may arise in part from subjective feelings," but he is quick to add that "subjective data may be objectivized and analyzed. Psychologists constantly treat subjective emotions scientifically."

Even if we were to concede that the "objective methods" of psychology will some day make possible on a large scale scientific control of the primitive forces of man's feelings and, thus, release new

energies for improving interpersonal relationships, what does this possibility, which is the behaviorist's dream, imply? It implies something that neither psychologists nor psychiatrists, of whatever school of thought, can settle by mere recourse to their special techniques.

If man's irrational behavior is to be controlled scientifically, the momentous problem that faces psychologists is, *Which* direction should the proposed conditioning take? No matter how they answer, what underlies any scheme of scientific control of man's irrational outbursts is a distinction between "normal" and "abnormal" behavior—a distinction that presupposes some "norm" or criterion of value. (Substitution of the statistical term *average* for *normal* does not change the logic of the situation here.) Just as the difference between a healthy and a sick body involves, strictly speaking, a moral distinction rather than a purely biological one—the pathological situation being as biologically conditioned as its opposite physiological state—so the difference between a healthy and a sick mind involves a moral distinction rather than a purely psychological one. Consequently, as soon as a school of psychology formulates its standard of normal behavior (for example, that of adjustment to the environment), it willy-nilly passes a moral judgment. It assumes (tacitly or expressly) in this case that adjustment to the environment is the *proper* goal of life and that maladjustment is *bad*. Now then, is not such a behavioristic criterion debatable in theory at least? The very fact that schools of psychology do not agree on what constitutes normality in human behavior is proof that the question is one that cannot be settled by mere "observation and experiment." Any system of psychiatry that denies the moral presuppositions of its therapeutic measures commits the fallacy of false premise, even if the premise itself is unavowed. A false premise is no less false because it is unavowed.

"Individual psychology," Emerson asserts, "is highly important to any general theory of ethics." This is true, of course, insofar as a scientific account of human motivation furnishes the raw materials of ethics. But his assertion is only half of the truth, since no clinical psychology can ever hope to resolve the conflicting desires of men without sooner or later making moral assumptions concerning what is desirable or undesirable. There is no way out of this *ethnocentric predicament* in matters of human conduct, and the wisest course for students of those sciences that border on ethics is to be completely candid about it and to proceed from the most reliable moral assumptions possible rather than to boast about being cold-blooded analysts—a pose that is as silly in principle as it is ridiculous in ac-

tion. In fine, a general theory of ethics is just as important to individual psychology as it is conversely.

So much for the author's first, or observational, principle of scientific method as it applies to ethics. With regard to his statement of its second, or theoretical, principle—"determination of causes and effects and formulation of theoretical interpretations in conformity to the facts and their relations"—first, the statement omits the significant rôle that creative imagination plays in the formation of scientific hypotheses. Second, the statement does not apply to pure mathematics as such. If you assume that the only legitimate form of scientific theory is one "in conformity to the facts and their relations," then a strictly formal discipline like pure mathematics could not be called a science, which is absurd. Not that Emerson fails to recognize the leading part that mathematics has played in modern science—far from it! I do not wish to belabor the point, but I am thoroughly convinced that a man's stand on pure mathematics is perhaps the best index to his general philosophy of science, for it is in the field of pure mathematics that a thinker is likely to show his true or false colors. A man whose philosophy of science fails to appreciate Euclid's power to look on "Beauty bare," is, in the long run, also likely not to appreciate Kant's power to look on "Duty bare," if we may carry Edna St. Vincent Millay's poetic insight into pure mathematics over to ethics proper. This is not to say that we should follow Kant to the letter and preach duty for duty's sake, but it is to say that we should no more discard his insights into the logic of ethics than we should discard Euclid's insights into the logic of geometry.

The last and most serious objection that I have to Emerson's statement of the theoretical aspect of scientific method has to do with its implications for ethics itself.

If one accepts Emerson's etiological conception of scientific theories or laws, then all ethics is necessarily restricted to an *instrumental* function and all its principles are reduced to *rules of prudence*. That is to say, all that could ever issue from the application of such a conception to the field of ethics is a series of hypothetical imperatives in the form of conditional propositions that would stipulate the means that are necessary to attain certain ends, or at the very most, stipulate the ends that are mutually compatible or incompatible. It is clear, of course, that in order to settle any question involving the realization of our objectives, a prior "determination of causes and effects" is necessary, but it should be equally clear that all the knowledge of causation in the world as to what steps are *appropriate* or must be taken to bring about a desired

effect is not sufficient to settle either the question of whether the steps are morally *proper* or the question of whether the desired effect is morally desirable. If this were not the case, the Machiavellian doctrine that the end justifies the means would constitute the quintessence of morality. In short, the means-end relationship that underlies the whole sphere of human conduct involves *something more* than the cause-effect relationship. This "something more," which is the missing link in all forms of utilitarian ethics, is the factor of *intrinsic* value.

Let us illustrate with the goal of human life that is set by Emerson's principle of homeostasis—"survival" or "efficient existence." If we want to survive, the prudent thing to do is to follow the rules of hygiene that are based on the findings of the medical or biological sciences. These findings neatly describe the conditions that we must satisfy in order to enjoy "optimal living." All of which doubtlessly makes perfectly good biology but not necessarily good ethics. "Optimal living," has only "survival value," which is *not* the same thing as moral value. In fact, from the standpoint of ethics proper, non-survival may sometimes be better than survival. What decent person would not prefer to die with dignity rather than to live at any cost under conditions of infamy? Such a preference would be impossible to establish on purely biological grounds, and I am confident that Emerson would concur, even though he rather cryptically states the difference between quantity and quality of life: "Quantity of life is not necessarily the same as homeostasis of life."

In any event, survival as such is ethically neutral; its basis is purely instinctive and becomes ethically significant only when we are in a position to determine the purpose that it is subserving. This applies equally to whatever is conducive to survival, including health, the main objective of the physician's art. Richard C. Cabot used to insist that health in itself is "morally neutral, as effective a tool for evil as for good" (7). A healthy scoundrel is no better—if anything, he may be worse—than an unhealthy one. In saying this, I am not discounting the fact that most psychologists and psychiatrists today would contend that a "healthy scoundrel" is a contradiction in terms. Still, their contention only serves to strengthen my point—that problems of health involve ultimate issues of an ethical nature. Incidentally, the same conclusion would be reached if we took any other interest, such as wealth, power, fame, and knowledge—and for the same reason, that the actually *desired* and the possibly *desirable* are not necessarily coextensive terms.

Emerson's biological variant of the utilitarian theory of ethics, which is implicit in his positivistic

conception of scientific method, receives explicit formulation in his application of the principle of homeostasis to the moral field. The homeostatic theory of ethics, inspired directly in Cannon (indirectly in Darwin), may be defined as the doctrine that maintains that the rules of prudence are the only reliable guides of life. What could be more conducive to survival than the knowledge of the stabilizing mechanisms that operate in the body and the exploitation of that knowledge for the adjustment of means to ends or the realization of our intended aims? Yet, even the great apostle of the Utilitarians, John Stuart Mill, was obliged to declare, perhaps in an unguarded moment, that "right action must mean something more and other than merely intelligent action" (8). Consequently, a principle derived exclusively from biological data, such as homeostasis, can provide only one-half of ethical wisdom, the *instrumental* half. The other half—I am tempted to add, the better half—must come from a principle that performs a *normative* function in the sphere of conduct—that is, one that specifies the ultimate standard for evaluating the means chosen and the ends desired.

However empirically valid the principle of "dynamic homeostasis" may be in biology, no extension of it from the biological to the moral field by any argument from analogy, such as Emerson submits, can ever transform its actual *existential* status into a *normative* principle. To expect such a morally neutral concept from physiology to serve as a "unifying principle" of both biology and ethics is to believe in a theoretical miracle. Here is where Emerson's whole appeal to "analogical reasoning" breaks down completely.

No valid analogy in the way of content between ethics and biology is possible if ethics is conceived properly—that is, as a normative discipline. If, on the other hand, ethics is not conceived as it should be, then any analogy it may have with biology and other allied sciences is quite beside the point. No social principle regarding facts alone (such as social homeostasis) can ever resolve value problems as such, since its self-appointed task is to interpret *existing* social conditions, not *ideal* ones, which by their very nature as ideal must always be postulated as better than actual conditions at any given period of time. Hence, what is needed to resolve strictly ethical problems is *ethical* theory, and for the same, if not the greater, reason, what is needed to solve strictly biological problems is *biological* theory. If biological problems, all of which refer to the same realm of discourse as physical problems, cannot seem to be made intelligible in purely physico-chemical terms, why should Emerson expect that social problems, some of which at least refer to a

different realm of discourse, may be made intelligible in purely biological terms?

The fact that we deny that biology and ethics are analogous in *content* or subject matter does not mean that we deny that the two disciplines are analogous in *form* or method of reasoning. All sciences, on reaching maturity, proceed to formulate their general principles in terms of what the logician or mathematician calls "limiting concepts" (for example, the frictionless engine, the perfect vacuum, and the ideal gas). Since all general principles by nature state what would be the case only under certain purely logical or abstract conditions, their function in scientific inquiry is clearly *regulative*; that is they enable us to put the flux of actual phenomena into some significant order and thus make our understanding of them possible. It seems to me that the trouble with the logic of Emerson's article is not that it rests on "analogical reasoning." There is nothing wrong with this! Rather, its logical difficulty arises from the fact that it mistakes the analogy between ethics and biology in their *limiting* concepts for an analogy in their *material* ones, thereby confusing analogy in method of reasoning with analogy in subject matter.

To see Emerson's mistake more sharply, just consider for a moment what the all-inclusive limiting concept of biology would be. Precisely, it would be *homeostasis*, inasmuch as this concept is descriptive of what would happen in the organic world if conditions of "optimal living" prevailed. Accordingly, optimal living is to biology what, say, frictionless motion is to physics and justice is to ethics. To the extent that all sciences in their advanced stages use limiting concepts for the formulation of principles—but only to that extent—can valid "analogical comparisons" be established among them. In a word, nobody is justified in jumping at the conclusion that biology and ethics are analogous *materialiter* from the fact that they are analogous *formaliter*. Had Emerson made a distinction between the two possible kinds of analogy (formal and material) pertinent to his comparative study, he would have been in a much better position to meet the perennial critics of analogical reasoning.

Another way to show the defect of Emerson's particular use of analogical reasoning is to consider the difference between the search for moral truth and the search for factual truth. Although all hypotheses in science aim at the truth, the truths of ethics are radically different, in part at least, from the truths of fact. Ethical propositions doubtlessly have a factual content, but the characteristic thing about them is their normative content. Although the search for factual truth involves "the formulation of theoretical interpretations in conformity

to the facts and their relations" the "search for ethical truth" calls for the formulation of theoretical interpretations in conformity to our *ideals* and *their* relationships.

In the social sciences, to take the area of study closest to ethics, we formulate hypotheses in order to understand what the social facts *are*, but in ethics proper we formulate hypotheses in order to understand what those facts *should be* in terms of human possibilities. Thus, the search for moral truth necessitates a mode of equilibrium with our environment that is opposite to that required by the search for factual truth. Whereas, in the latter case, our interest is in making our ideas conform to the environment, in the former case, we are concerned with making the environment conform to our ideals. In other words, the whole purpose of an ethical hypothesis is not to stick to the facts; rather, its purpose is to effect a change in the facts of our environment, physical and social, so that the results will be more in keeping with the ideals we cherish. After all, the injunction to practice what we preach means precisely not to live altogether "in conformity to the facts and their relations." As in moral life, so also in moral theory. To conclude in Emerson's terminology, what is needed in the way of theory in the field of ethics is not a *biological* but an *ethical* principle of "dynamic homeostasis."

This conclusion is not meant to deny that everything that the various descriptive sciences—physical, biological, psychological, and social—can teach us about man and society is pertinent to ethical inquiry. There is no doubt in my mind that an adequate system of ethics must take into serious account all the fundamentals known about man in present-day science. The whole value of Emerson's argument for a "biological basis of social ethics" and of his accompanying plea for a "scientific approach to ethics" lies precisely here. Being a philosophic naturalist myself, I certainly have no intention of minimizing the great significance of his plea. Yet, granting the relevance of the available scientific data on the subject, the crucial question of their precise importance to ethics still remains.

The bearing of the descriptive sciences on ethics may be determined by considering each of the two aspects of the means-end relationship that underlies all human conduct. Taking up its instrumental aspect first, to the extent that the choice of effective means for achieving our ends depends on empirical knowledge of the causal connections of things in the world, a reliable theory of ethics must consult the various sciences for the pertinent information. Thus, viewed from the instrumental side, the value of the descriptive sciences in the moral field is definitely *positive*, serving as the basis of all intelligent conduct. However, as far as the other aspect

of the means-end relationship is concerned, which has to do with both the choice of the *proper* means and the choice of the *proper* ends, their value is chiefly *negative*, and this is in two senses. On the one hand, all the descriptive sciences serve to remind us constantly of our human limitations; some of them, such as history and anthropology, do so by concretely pointing out the relativity of our moral judgments. But, on the other hand, although all these sciences can tell us the goods or ends of life that are unattainable for a limited being like man, they cannot tell us the attainable goods or ends that should be pursued to achieve a maximum of values. Thus, the descriptive sciences provide the *necessary* condition, but not the *sufficient* condition, for a scientific treatment of ethics.

To illustrate the general thesis, we learn essentially from modern biology, that, since man is an animal, one cannot expect him to be an angel. *Ergo*, an ethic for angels will not do! This is a sobering lesson, and it goes without saying that it would be quite convenient for all of us if biology could, in addition, supply a positive ethic for man. But this is expecting a little too much of any science that treats only facts. Ethics, alas, cannot be cut to fit the special interests of the biologist. Or to paraphrase an old saying: There is no royal road to ethics, either!

To sum up our analysis of the second of Emerson's three "essential principles of the scientific method" from a strictly logical standpoint, why is not a biological basis sufficient for the construction of an adequate theory of ethics? The reason is substantially this: No conclusion on how we *ought* to live or conduct ourselves can be inferred from premises that are restricted to what *is*. Or positively stated, to have an "ought" in a conclusion, there must be at least an "ought" in one of the initial premises. Since biology is an existential science, all its premises are limited to "is" propositions about life, and, as a consequence, no system of biology can contain an "ought" in any of its conclusions. On the other hand, if a system of biology assumes (illicitly) some "ought," then it is no longer what it claims to be, but automatically becomes a system of ethics and must be judged as such. In other words, a system of biology cannot function at the same time as a system of ethics, since any attempt to carry it through would necessitate going beyond judgments of fact and appealing to judgments of value. This is precisely the dilemma that faces any system of biology that purports to serve as a basis of ethics. In short, a "unifying principle," such as "dynamic homeostasis," can succeed in the unification of the biological and the ethical only by eliminating their difference, either by denying the ethical outright, or, as in Emerson's case, more

subtly, by reducing it to a species of the biological.

According to Emerson, the third and final principle of scientific method is "the verification of relevant facts." Is this an adequate idea of the verification factor in scientific method as a whole? And is it applicable to the particular field of ethics proper? My answer to both questions is *No*. Let me explain why by making a few brief comments on the problem of verification in general and as it applies to ethics.

First, strictly speaking, hypotheses alone are susceptible of verification in science; "relevant facts" are subject only to observation. Second (again in the strict sense), there are no "facts" at all, relevant or irrelevant, to verify in the field of pure mathematics; its content is purely abstract. Third, all the "relevant facts" in the world would not be sufficient to verify a hypothesis in a normative field like ethics. In general, granting that, in order to be subject to proof (or disproof), a hypothesis must lend itself to some sort of test that will either verify (or falsify) it, the *appropriate* test that would be required is going to depend on the specific nature of the subject matter with which the hypothesis is concerned. This means that what constitutes *evidence* in the realm of knowledge is a variable, not a constant.

In order to understand the difference in *principle* between the problem of verification in ethics and its counterpart in the natural sciences, let us first note how any hypothesis about the external world is verified. Everyone knows that we determine the validity of a physical or biological hypothesis by putting it eventually to the empirical test of "sensory perception," which amounts to ascertaining whether its deducible consequences actually hold in fact. The recent controversy in the Soviet Union over Mendel's theory of heredity is a vivid case in point. Even Lysenko's comrades are beginning to realize that the fate of his neo-Lamarckian alternative to Mendelian genetics does not rest in the lap of their party, but depends on the weight of the empirical evidence. At all events, the natural sciences are in an enviable position with respect to methodological matters, because they can establish their hypothetical deductions and probability predictions on the basis of direct observations and repeated experiments—so much so, that it can be said that the final test of truth in the field of natural science is *warranted predictability*.

Why are the usual empirical or experimental tests not good for validating hypotheses in the field of ethics proper? Because such tests can be applied only to a cognitive situation where predictions or claims on matters of fact are made. Consequently, the truth of hypotheses that make no factual claims, like those of ethics proper, cannot be de-

terminated by the kind of test that is accessible to hypotheses that make such claims. What distinguishes an ethical from a factual hypothesis is the fact that the object of the former is to justify the *relevant ideals* that we should live by in order to achieve a maximum of values; thus, the question of "the verification of relevant facts" is quite irrelevant to its purposes. An ethical hypothesis does not yield *predictions* of fact that refer back to a world that *is*; rather, it yields *prescriptions* of value that look ahead to a world that *is-not-yet* and, strictly speaking, never will be completely achieved, since no ideal of life can ever be fully realized. Since this is the case, it follows that the social ideals that we project into the future, no matter how much they may originate from biological and social needs, cannot be tested simply against the ascertained facts of society, as positivists in general maintain; nor can they be tested against the ascertained facts of biology, as Emerson and his evolutionary school of thought presuppose. Those who argue this way not only commit the genetic fallacy on a grand scale by making the origins of morality determine the validity of our ideals, but also beg the ethical question by taking for granted that a test for truths of fact is also a test for truths of value.

Although empirical or experimental tests alone cannot determine the *truth* of our systems of ethics, they may determine their *falsity*. This occurs whenever an ethical system is based on some false existential premise; that is, one that is contrary to fact. Consider for a moment an example taken from Emerson's article.

It is now well recognized that a scientific anthropology can invalidate the Nazi conception of race relationships by showing empirically that it harbors a "false theory of racism." But, and this is not so well recognized, a scientific anthropology cannot validate a democratic conception of race relationships without assuming the desirability of the ideal of democracy itself. All the competent anthropology in the world could never settle the question of the validity of the democratic ideal by mere recourse to the empirical evidence on races found in biology. To be in a position to determine rationally the validity of rival hypotheses in the field of morals, something other than the empirical test of warranted predictability is needed. Since it would take us too far afield to discuss the sort of test that is needed for validating moral hypotheses, it is enough to suggest, as a final idea, that the normative test of *warranted desirability* is to ethical truth what the empirical test of warranted predictability is to factual truth.

Let me now draw together the various strands

of this critique of Emerson's attempt to introduce the scientific method into the moral field. Any proposal that calls for the application of the scientific method to ethics, in order to be at all effective, should proceed to designate the *specific form* of that "logical method" that is appropriate to ethical inquiry. In other words, if all scientific method, regardless of subject matter, involves certain prerequisites for the attainment of reliable knowledge—observational, theoretical, and verificational—then the task of any attempt to apply that method to ethics would be to elucidate exactly what those prerequisites are within the *ethical* context. This is no easy task, of course. Whatever may be my opinion of its ultimate success or failure, Emerson deserves much credit for his utter sincerity in attempting a scientific approach to ethics (9).

References and Notes

1. For the earlier contributions to the ethics of evolution, consult W. F. Quillian, Jr., *The Moral Theory of Evolutionary Naturalism* (Yale Univ. Press, New Haven, Conn., 1945).
2. A. E. Emerson, *Sci. Monthly* 78, 67 (1954). Unless otherwise noted, all quoted material in the text is from this article.
3. The present critique of the homeostatic theory of ethics would be applicable, *mutatis mutandis*, to all types of descriptive ethics, not just to the type based on the data of biology. Insofar as ethical theory is concerned, it really makes no significant difference whether moral questions are reduced to social, psychological, biological or even to physical terms. The crucial point at issue is whether it is legitimate, for purposes of ethical understanding, to reduce moral questions to *any* factual terms at all, whether or not they are biological. A recent volume that comes to grips with the problem here, insisting rightly throughout on the need to make the proper methodological distinctions between the factual and the normative disciplines, is *The Logic of the Sciences and the Humanities* (Macmillan, New York, 1948) by F. S. C. Northrop of Yale. However, one may seriously question whether Northrop actually satisfies that need in the course of developing his own position.
4. For a clear and pioneer statement of the emotive theory of ethics from the pen of a British "logical positivist," see A. J. Ayer, *Language, Truth and Logic* (Oxford Univ. Press, New York, 1936), Chap. VI.
5. The direct sources of the homeostatic theory of ethics appear in W. B. Cannon, *The Wisdom of the Body* (Norton, New York, 1932), pp. 287-306, and in his follow-up paper, "The body physiologic and the body politic" [*Science* 93, 1 (1941)]. For a striking anticipation of that theory in more Darwinian language, compare C. D. Leake, "Ethicogenesis" [*Sci. Monthly* 60, 245 (1945)].
6. J. S. Mill, *Utilitarianism* (Liberal Arts Press, New York, 1948), p. 10.
7. R. C. Cabot, *Adventures on the Borderlands of Ethics* (Harper, New York, 1926), p. 94.
8. J. S. Mill, *Nature* (Univ. of Oregon Cooperative Store, Eugene), p. 9.
9. I am grateful to C. D. Leake for his careful reading of the manuscript and for his helpful comments, although it must be added in fairness to him that he agrees on the whole, with the point of view that is criticized here.

BOOK REVIEWS

Advances in Virus Research. vol. II. Kenneth M. Smith and Max A. Lauffer, Eds. Academic Press, New York, 1954. x + 313 pp. Illus. \$7.

Volume II of this annual publication is an important addition to the virus literature, and certain articles will be of interest beyond the bounds of virology. In spite of the announced intention of the editors to discuss all types of virus from many points of view, one finds that more than half of the present volume is devoted to papers in the ill-defined field of biophysics, perhaps reflecting editorial prejudice. This is not a criticism of the book, because the biophysical chapters are among the more interesting.

The first chapter by F. O. Holmes deals with the genetic control of resistance to virus diseases in plants and is largely a catalog of breeding experiments. It is astonishing how little information is available on the mechanisms of resistance to plant viral diseases.

The rather limited literature dealing with inhibitors of plant viruses is next discussed by F. C. Bawden. He concludes that most inhibitors alter the host in such a way that the probability of infection is decreased rather than virus multiplication being affected. There seems to be no clear-cut evidence for effective chemotherapy of plant virus diseases.

The incomplete forms of influenza virus present an interesting puzzle to students of virus multiplication. P. von Magnus, in summarizing the literature on this subject, concludes that incomplete virus probably represents a developmental stage, although admittedly this has not been proved. Much is known of the conditions that lead to incomplete virus formation, but generalizations are scarce.

The characteristics of viral development in isolated animal tissues are rather briefly discussed by Ackermann and Francis. The use of various metabolic inhibitors has permitted the isolation of successive stages in the reproduction of certain animal viruses, something that would be difficult to accomplish in the intact animal. Such studies may yield useful information concerning the metabolic factors that control virus reproduction.

The next paper by E. Pollard presents the point of view of the Yale group on the action of ionizing radiations on viruses. The effects of these radiations on various physiological properties of well-known viruses are being studied with the expectation that powerful analytic tools may be developed for the rapid definition of structure in unknown viruses. This very interesting chapter is, unfortunately, marred by a number of mathematical and typographic errors.

The chemical constitution of viruses is reviewed by C. A. Knight. Nothing very surprising has been discovered so far in the composition of viruses, except for the unique kind of nucleic acid found in the bacteriophages related to T2. However, one is struck by the very limited range of virus types that have been

purified and analyzed. One suspects that there may yet be plenty of gold in the hills.

The next chapter is an extensive, critical review of the electron microscopy of viruses in the beautifully lucid style that we have learned to expect from Robley Williams. This chapter is heartily recommended to anyone who is interested in the biological applications of the electron microscope, or who has been disturbed by an author's interpretations of his micrographs. One suspects after reading this paper that we should all have been frequently disturbed.

The concluding chapter by Lauffer and Bendet deals with the esoteric subject of the hydration of viruses. This is the most complete and detailed discussion of the subject available and, although of great interest to biophysicists, it is not likely to arouse much enthusiasm among virologists.

Although certain chapters in this volume are of very restricted interest, the book as a whole may be expected to be of value in teaching and research throughout the range of the biological sciences.

MARK H. ADAMS

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From Classical to Modern Chemistry. Some historical sketches. A. J. Berry. Cambridge Univ. Press, New York, 1954. xi + 251 pp. \$4.75.

This is a historical account of chemical science, continuing the treatment by the same author in *Modern Chemistry—Some Sketches of Its Historical Development* (1946). Theoretical chemistry is traced in an introductory chapter from Cavendish through Ostwald, Nernst, and Davy to the present. Special attention is given to conflicting concepts of matter and the relationships between physics and chemistry. This is followed by eight chapters on separate specific subjects.

A chapter on heat theory ranges from the early experiments of Lavoisier, Rumford, and Carnot to the concepts of thermodynamics, entropy and free energy, quantum theory, and so forth.

Electrochemistry is introduced by an account of the early experiments in static electricity, followed by a review of the works of Helmholtz, Faraday, Kelvin, and Nernst. Classical and modern theories of ionization conclude the chapter.

Physical optics in relation to chemistry is treated by an account of Newton's experiments, Fresnel's concept of molecular refraction, and the relationships later disclosed between molecular configuration and the interactions of light with matter. The chapter concludes with a brief treatment of Einstein and de Broglie's reconciliation of the wave and corpuscular theories of light by means of the quantum theory. This is briefly exemplified by the Raman effect.

A separate chapter is devoted to the development of

ideas concerning molecular magnitudes. Beginning with Thomas Young's studies on capillarity, the discrete structure of matter is traced through the work of Graham, Loschmidt, and Rayleigh on gases to the related observations on the unit electronic charge by J. J. Thomson and Millikan. The chapter concludes with inferences on molecular magnitudes derived by du Nouy and Langmuir from the spreading of glyceride oils on water.

Analytic chemistry is introduced by the development of Bunsen and Berzelius. The physicochemical outlook is observed in the studies of indicators, volumetric methods, and the beginnings of gas analysis. The contributions to elementary organic analysis by Liebig, Dumas, Kjeldahl, and so forth, are recounted. The development of microanalysis by Emich and Pregl and of chromatography by Tswett completes this subject.

The next two chapters treat the subjects of chemical formulas, valency, and molecular configuration. The conflicting concepts of Berzelius and of Gmelin regarding combining weights are used to illustrate the importance of Cannizzaro's formulations based on Avogadro's principle. Early concepts of valency are attributed to Frankland, and their development by Kekule and others is traced. Loschmidt is mentioned as the first to introduce graphic formulas for organic compounds in 1861. The work of Werner on complex salts and of Gomberg and Paneth on free radicals leads to the concepts of electrovalency and covalency. Modern electronic theories expounded by Lowry, Sidgwick, and others conclude this chapter.

The final chapter deals with chemical kinetics—preparation of pure compounds, temperature effects, catalysis, and reaction mechanisms—associated with the names of Armstrong, Moureu and Dufraise, Bodenstein, Nernst, Hinshelwood, and others.

The book is written in an entertaining, if somewhat sketchy, style. The casual reader should have an elementary acquaintance with chemistry, but the serious student will find generous references to the literature at the end of each chapter as well as a brief bibliography of the general histories of chemistry.

LUCIUS W. ELDER, JR.

Central Laboratories, General Foods Corporation

Animal Form and Function. An introduction to college zoology. W. R. Breneman. Ginn, Boston, 1954. viii + 488 pp. Illus. \$6.

The writer of an elementary textbook can expect no mercy from his colleagues. Not only does he invite their hostility wherever he trespasses, as of course he must, upon fields other than his own, but he is sure, too, to offend against the principles and prejudices of pedagogic method that are the cherished expressions of the elementary teacher's individuality.

I do not think, however, that Breneman's book calls for detailed, let alone spiteful, criticism. It makes a few mistakes; the general excellence of the illustrations is occasionally betrayed; and some of the omissions are odd. Nonetheless, it clearly succeeds very well in

doing what the author wanted it to do—that is, to meet the needs of the student taking a short course in zoology, but not necessarily with the intention of carrying this study further.

For such a student the old "type" system of instruction is peculiarly inappropriate, and this book shows, in my opinion, just the right approach. On the other hand, it fails, in company with nearly every elementary zoological textbook, to take the reader sufficiently into its confidence in regions of modern inquiry where difficulties, uncertainties, and differences of opinion prevail. Of course, one must not overdo the treatment of such matters when introducing people to one's subject. They will, however, sympathize with us and respect us more if we tell them something of the problems currently exercising our minds and of the difficulties we face in solving them.

This said, the fact remains that we have been given an extremely attractive introduction to the animal kingdom and to zoology.

D. R. NEWTH

Department of Zoology, University College London

My Way of Becoming a Hunter. Robert H. Rockwell with Jeanne Rockwell. W. W. Norton, New York, 1955. x + 285 pp. Illus. \$3.75.

Habitat groups in great American museums are among the nearly perfect human accomplishments. The animals, whether mounted or modeled, seem as nearly like living nature as anything can without sound or motion; and vegetation, rocks and painted backgrounds are wonderfully done. Robert Rockwell's career has been collecting for and preparing such exhibits of large mammals. This, his autobiography, tells amusingly of his boyhood in Ireland, of his job as a taxidermist in a commercial establishment in New York and his training gained at Ward's in Rochester. Then followed positions at the U.S. National and Brooklyn museums. The latter involved expeditions to the Adirondacks for deer, to Nova Scotia for caribou, and to Alaska for bear, sheep, and moose. He spent 16 months on the amazingly ill-conceived and ill-managed voyage on the ill-found schooner *Blossom* to the South Atlantic. Then he joined the American Museum of Natural History and went to Africa with that great master Carl E. Akeley to collect large mammals, which he spent the next 11 years mounting. After retirement, Rockwell made trips to Alaska for private parties.

That the book is naively written and could benefit by editing will not keep many who have derived pleasure and instruction from habitat groups, those with anything of the primitive hunter in them, and those who thrill at real adventures, from enjoying it. Also, there are many informal glimpses of prominent persons. The illustrations are 2 dozen photographs of the author's finished groups, of scenes from his acquiring and mounting of the animals, and of charming small bronzes by him.

CHARLES H. ROGERS

Princeton Museum of Zoology, Princeton University

A Manual of the Dragonflies of North America (Anisoptera). Including the Greater Antilles and the provinces of the Mexican border. James G. Needham and Minter J. Westfall, Jr. Univ. of California Press, Berkeley, 1955. xii + 615 pp. Illus. \$12.50.

It is always a pleasure to see a well-executed and beautifully prepared book such as this one devoted to a group of insects. A great many of the illustrations are fine photographs and are a great improvement over the rather crude drawings that illustrated the predecessor of this work [James G. Needham and Hortense B. Heywood, *A Handbook of the Dragonflies of North America* (Thomas, Springfield, Ill., 1929)]. There are 341 figures, many of which are photographs of nymphs or of male genitalia; others are drawings of wings or other structures. The taxonomic part will greatly facilitate identification of dragonflies. It is to be hoped that this, together with the introductory sections on dragonfly biology, collecting, and methods of preservation and study, will introduce many people to this insect group and stimulate further study. The language is simple and the technical words are clarified by illustrations and a glossary, so that the interested amateur can use the book.

In spite of its excellent qualities, it contains some faults. Localities are rarely indicated except by state. In the West particularly, this means little. One would not know where to go to find a species recorded from "Calif." or "Tex." It would have taken very few more lines to indicate something about the area where each species occurs.

The authors seem to have taken no cognizance of the advances in the philosophy of systematic zoology that have occurred in recent decades. The species concept is purely morphological. Geographic variation is ignored or, more likely, some of the "species" are in reality geographical variants of others. One repeatedly finds that a form regarded as a species in keys and headings is, in the text, said to be a "variety" of another species. The true status of such forms is obscure, although it is obvious that the authors could have formed sound judgments in at least some cases.

CHARLES D. MICHENER

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Conserving Natural Resources. Principles and practice in a democracy. Shirley W. Allen. McGraw-Hill, New York-London, 1955. ix + 347 pp. Illus. \$5.50.

The newest of some half-dozen American textbooks on general conservation is also one of the best. It combines the thorough coverage of those that have been written by collaborating specialists with the sustained and uniform quality that comes from single authorship. It also has flavor and character, being both salty and reliable.

Any student who reads it will have a well-balanced and realistic understanding of the resource problem as it has developed in the United States. This, of course, he has a right to expect from a general textbook. But he will also get a feeling for the spirit and mentality

that have made the U.S. Forest Service almost unique among our public institutions. This amounts to a kind of tolerant doggedness, an ability to give with the punch and come back grinning, ready to keep going.

Why bring in the Forest Service? Simply because S. W. Allen was brought up in its discipline and is a worthy exemplar of its tradition. And if that tradition is basically empirical, carrying on the best it can without too much concern for the higher finesse of its own philosophic background, I for one will not object. I revere the human intellect and its eternal quest for ultimate values, but in the rough and tumble of democratic living I would rather trust the blunt and practical man whose sympathies are warm and whose instincts are sound than the philosopher who is out of touch with the realities of life.

PAUL B. SEARS

Conservation Program, Yale University

Mammals of California and Its Coastal Waters. Lloyd Glenn Ingles. Stanford Univ. Press, Stanford, Calif., rev. ed. 1954. xiii + 396 pp. Illus. + plates. \$6.

This is a revised edition of Ingles' 1947 work, *The Mammals of California*. As it was in the earlier version, the style is lucid and highly readable, somewhat more so, as a matter of fact, than is usually found in books of this sort. There are brief sections on mammalian orders, fossil history, why and how to study mammals, accounts of the California species, and extensive appendixes. With the exception of the enlarged section on the Cetacea, the species accounts are essentially the same as in the earlier edition. The other sections are either new or completely rewritten and enlarged. Subspecies are omitted.

The synopsis of mammalian fossil history is too brief even for a book of this nature. As often happens in textbooks for beginners, excessive brevity and its attendant over-simplification are as likely to mislead as to inform, and I should have preferred a more sophisticated treatment or none at all. *Why* mammals are as they are, are where they are, and do as they do may often be suggested or solved by the fossil record. And surely the *why* is as important, even to the beginner, as the *what*. Ingles sets forth his concepts of conservation and control under the heading "Why we study mammals." His views may be summarized by his statement:

Conservation practice implies intelligent action and, where any vertebrate animal is involved, should first be preceded by a planned research for knowledge about its life history, its ecology, and its population phenomena (p. 26).

Assuredly this is an unassailable position. Sound conservation practice does indeed require intelligence and a sound sense of values as well as knowledge. Too often scientists seem to infer that only knowledge is essential. The acrimonious debates on conversation and control practices often owe their genesis to the illusion that scientific knowledge constitutes, rather than forms the basis for, a value judgment.

The species accounts are essentially natural history

accounts with a minimum of descriptive and no systematic material. Not all the species of California mammals have been honored by an account. Apparently the ones that were chosen are from among the larger, the more widespread, or the more spectacular forms. Many species are mentioned only in the check list or in the pictorial species key. This latter consists of black-and-white drawings in the Roger Tory Peterson style accompanied on a facing page by a telegraphic description and distribution notes. The drawings in the keys, and elsewhere for that matter, are of indifferent quality. Most are appealing enough esthetically but not scientifically; indeed, some are remarkably inaccurate and, together with the inferior reproduction of the halftones, are the weakest feature of the book. For example, the ears of the pinyon mouse (p. 222) and of the Norway rat (p. 230) are disproportionately small, as can be ascertained from photographs of the living animals on pages 225 and 239, respectively. Nonetheless, the technique of identification by means of annotated pictures is an effective one and worthy of use. There is also a usable separate key for the identification of skulls of California mammals to genus.

There are more kinds of mammals in California than anywhere else in the United States. A definitive technical treatise on that state's mammals is badly needed and long overdue. Clearly, however, the Ingles book is not intended to fulfill that need. It is intended rather for the beginning student in California and for the interested lay person. Like the earlier edition, it should satisfy the reader for whom it is intended. More is neither asked nor given.

KEITH R. KELSON

National Science Foundation

The Material Culture of Pueblo Bonito. Smithsonian Miscellaneous Collections, vol. 124. Neil M. Judd. Appendix, Canid Remains from Pueblo Bonito and Pueblo Del Arroyo, Glover M. Allen. Smithsonian Institution, Washington, 1954. xii + 398 pp. Illus. + plates. \$5.

Pueblo Bonito is the name given to one of the largest urban developments in ancient North America. The pueblo ranged from one to four stories in height. On the ground floor alone were some 300 rooms; and in all, there were probably many hundreds of rooms, although not all simultaneously occupied. The earliest part of the pueblo was built early in the 10th century, and the last roof beams were cut in A. D. 1130. The population numbered at least 1000. There are about 12 other sites nearby in Chaco Canyon that are roughly comparable in size and age. This area was densely populated, perhaps too much so.

The excavations at Pueblo Bonito were carried on from 1920 to 1927 and rank as one of the three or four largest ever undertaken in the Southwest.

Ever since the latter part of the 19th century, when Pueblo Bonito and the other nearby sites were first brought to the attention of the public, archeologists have been eager to recover the complete history of these

large sites. Therefore, when Judd was appointed chief of the Pueblo Bonito Expeditions of the National Geographic Society, everyone rejoiced, for an archeologist worthy of this great task had been selected. It should be noted that George H. Pepper had conducted excavations at Pueblo Bonito for the American Museum of Natural History (as a part of the Hyde Expeditions) from 1896 through 1899. Pepper's work and report, published by the American Museum in 1920, aroused great interest in the Southwest in general and in Chaco Canyon in particular.

Judd's report, then, is the second largest and most detailed report to appear on the archeology of Pueblo Bonito and the third in the series on the Pueblo Bonito Expeditions of the National Geographic Society. Judd's report has been awaited by his colleagues, and we were not disappointed.

The materials and information that he recovered are described under "functional" categories: "Subsistence and living conditions"; "Implements of the field and chase"; "Objects of religious implication"; and the like. This is a useful and pleasant arrangement and adds zest for the reader. In fact, I found the report absorbingly interesting. I read it through with delight and amazement, for I had forgotten that expository narrative could be so satisfying. I know of no archeological report of recent years that can be compared with it for graceful diction and careful phrasing.

It is generously illustrated; appendix A gives important identifying data on material illustrated. Among mechanical aids that are lacking, I would have found most useful an expanded table of contents, a list of illustrations, plans and sections, frequency tables showing occurrences of objects by time levels, and a more detailed index. But perhaps forthcoming reports, as are promised, will provide these. Judd mentions that one subsequent report will consider the *exceptional* pottery. I should strongly recommend that such a report cover *all* pottery from Pueblo Bonito recovered by the expedition.

The National Geographic Society should be pleased with its labors; and the Smithsonian is undoubtedly proud to include this report in its series. Thanks to an old friend who bucked all odds to finish this report.

PAUL S. MARTIN

Chicago Natural History Museum

Paracas Fabrics and Nazca Needlework, 3rd century B.C.-3rd century A.D. Junius Bird and Louisa Bellinger. National Pub. Co., Washington, D.C., 1954 (for the Textile Museum, Washington). vii + 126 pp. Illus. + plates. \$18.

It is not often that catalogs of collections in museums are as scholarly, well-written, informative, and superbly illustrated as *Paracas Fabrics and Nazca Needlework*. Instead of publishing pictures of all the 600 Peruvian textiles collected by George Hewitt Myers in a single volume, the Textile Museum of Washington, D.C., decided to limit this study to the 90-odd aboriginal specimens from the Paracas Peninsula, with a few related

pieces from Nazca, of the south coast of Peru. Future volumes will cover other areas and culture periods of Peru.

In engaging the services of Junius Bird of the American Museum of Natural History, one of the leading authorities on Peruvian archeology, and Louisa Bellinger, curator analyst of the Textile Museum who directed the technical studies, to prepare this book, the Textile Museum not only made a superb choice of highly qualified coauthors but demonstrated the fruitful scientific results that can develop from the collaboration of experts on a complex subject. This book should prove of interest, not only to archeologists and textile technicians specializing in Peruvian cultures, but also to anthropologists in general, museum curators, modern textile experts, and art historians. It should become one of the basic reference books on the art of weaving.

Although the illustrations and detailed descriptions refer only to those specimens actually in the collections of the Textile Museum, comparative data mention other known specimens and often offer the valuable information of stating where other fragments of the same specimen are to be found. The report is organized with an introduction by Bird that briefly outlines the archeological history of coastal Peru with an effort to place the textiles of the Paracas-Cavernas, Paracas Necropolis, and Nazca Valley sites into a time sequence. Although Bird's treatment of the chronology is good, generalized statement on a subject that is constantly fluctuating as a result of increased activity in Peruvian archeology, some Peruvianists will agree wholeheartedly with his statements and dates and others will take decided exception. These differences, however, in no way interfere with the textile analysis and conclusions.

The catalog is divided into detailed discussions of items of costume, such as mantles, ponchos, skirts, turbans. The design of Paracas textiles are then broken down into subdivisions of birds, snakes, mice(?), cats, people, winged men, mythological people, and monkey-footed creatures; this is followed by a discussion of the various techniques used in executing the cloth and designs. A description is then given of the few Paracas-Cavernas and related Nazca textiles in this collection. Perhaps the most rewarding chapters to the textile expert are the technical charts and the discussion of their meaning by Bellinger. This section is highly detailed but so clearly expressed and with such good diagrams that it is invaluable to the nontextile expert. The technical data are summarized on a chart broken down into three main categories: (i) ground fabrics, with the information tabulated according to materials (warp and weft fibers, yarn make-up, and so forth), fabrication (weave, braid, single element); (ii) embellishment, subdivided into materials (paint and yarn) and application (pattern, fringe, finish); and (iii) function.

Too often the colors of Peruvian textiles in reproduction are harsh and gaudy; the 14 color plates in this volume are an outstanding exception. The Textile Museum is to be congratulated for insistence on accuracy from the engravers and printers. The 113 black-and-white plates are excellent, especially the enlargements showing details of construction. Cross-referencing of

the plates throughout the entire book is well done and lends to easy reference, regardless of the reader's approach.

Bird feels that much more can be learned from textiles than from any other class of artifacts outside of written textbooks. Although some archeologists would argue the point, especially those who specialize in pottery, it must be admitted that the data and interpretations as presented in this volume by Bird and Bellinger are quite convincing of such a point of view.

CLIFFORD EVANS

Division of Archeology, U.S. National Museum

An Introduction to Psychology. Harry W. Karn and Joseph Weitz. Wiley, New York; Chapman & Hall, London, 1955. xi + 315 pp. Illus. + plates. \$3.90.

In the preface, the authors state that "This book is intended primarily for students whose academic exposure to psychology may begin and end with a one-semester introductory course. . . ." In one rather short volume, the authors have attempted to present the essentials of the science and its applications. The 13 chapters run from a well-presented if somewhat outdated discussion of scientific methodology in psychology through such usual topics as perception, learning, personality, individual differences and their measurement, and finally persistent problems of college students. There is no separate chapter on motivation, as is announced in the preface, although motivation and learning are paired up in one chapter. The motivational theory, previewed in the chapter on the basis of behavior, is the general homeostatic one that recently has been the subject of rather heated attack by theorists.

This book, when used as a textbook for beginning students, will require an extraordinarily good teacher. There is no apparent, underlying system to which the chapters relate, and the bulk of the chapters seem put together as separate essays rather than as steps to a goal. It seems strange and unfortunate that the chapters on individual differences and their measurement should follow the chapters on personality, as if personality and ability were separate domains. Unless given the benefit of presentation by a thoughtful teacher, the book might be confusing to the beginner. There seems occasionally to be an underemphasis on theory, as in the discussion of the characteristics of measuring instruments, which is both extremely brief and, considering recent developments, somewhat out of date. Reliability does not, for example, refer just to consistency. Rather, it refers to the amount of error present in a measuring device. On the other hand, the description of engineering psychology is a welcome addition, particularly for student engineers who will be designing machines that must still be operated by human beings.

On the whole, however, and especially in the hands of a good teacher, this should be a useful book for the students for which it was written. Its appearance raises again the question of the desirability of special courses for terminal students, a situation faced in other sciences as well as in psychology. The student going on will al-

most certainly have to study more general psychology before he can progress into advanced courses. How satisfactory an exposure to psychology this book will provide for the student of engineering or education remains to be seen.

JOHN W. GUSTAD

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Charles Darwin and the Golden Rule. The late William Emerson Ritter. Edna Watson Bailey, Ed. Science Service, Washington; Storm, New York, 1954. xxi + 400 pp. \$5.

A deep bond of sympathy exists between William E. Ritter, of the generation before mine, and me. That sympathy begins with our valuation of our Wisconsin farm backgrounds and extends to our deep conviction that natural history is the foundation of biology. We are in agreement that the naturalist may retain an emotional enthusiasm for his subject matter, an enthusiasm that is sometimes thought to be foreign to the spirit of science. We agree in rejecting supernaturalism and dualism, whether in philosophy or religion. And we agree in thinking of evolution as the central theme of the biological superstructure of subspecies, even when it is forgotten or neglected in the laboratory.

Ritter's career commands the respect of the succeeding generation of biologists for his influence on one of the great American patrons of science. That influence is crystallized for the modern world in the Scripps Institution of Oceanography and in Science Service, which have the separate but related functions, in James Smithson's fine phrase, of the "increase and diffusion of knowledge." *Charles Darwin and the Golden Rule* is dedicated to E. W. Scripps and memorializes the relationship between the two men.

The content of Ritter's posthumous work is his confession of faith, which involves an account of his own religious development and his personal reconciliation of religion and science. It is still more directly concerned with the relation of the theory of evolution with the problems of ethics. To these ends he examines and interprets Darwin's *Origin of Species*, *The Descent of Man*, and *Expression of the Emotions in Man and Animals*. In the second part of the book he concerns himself with religion freed from supernaturalism and with mysticism as a resource of religion to the scientist. I find little with which I cannot heartily agree.

With so much predisposition to like Ritter's book, I am a little unhappy to confess that I do not. The title might lead one to think that it is mainly about Darwin's attitudes toward ethical problems or else about the impact of Darwin's work upon ethics and religion; it is disappointing, then, to find that it is about Ritter, rather than about Darwin, and that his discussion of the impact of Darwinism on ethics is mainly of the impact on Huxley. Tilting at Huxley's unfortunate emphasis on the struggle for existence at the level of "nature red in fang and claw" is dated, for the insight of our generation has been into the selection of whole populations and of whole societies. Nowhere does Ritter

seem to understand that the very moral qualities in the human species (of which he writes at length) may have their origin in their development by natural selection for those very qualities, which are essential to a social and cooperative species. Darwin, in fact, saw this clearly and comments upon it in his treatment of the origin of the social insects.

Ritter writes from the viewpoint of an older generation, without having been able to bring his thinking upon the basic relationships of religion and science into consonance with modern developments. His bibliography extends to 1938; but he had not encountered the work of W. C. Allee on unconscious cooperation, or *proto-cooperation* (Allee, 1931, *Animal Aggregations*, 1938, *The Social Life of Animals*).

I have examined his bibliography critically: he has failed to include what seem to me to be the most important of the older thinkers who have wrestled with precisely the same problems. I miss most of all Hume and Spinoza. Spinoza offers the basis for the very concept of the relationship of man and the universe for which Ritter is in search. William James' *Varieties of Religious Experience* is cited; but not *Pragmatism* or the illuminating essays in *Memories and Studies*. Ritter quotes from Hudson's merely fantastic *Green Mansions* but not from the profoundly poetic *Far Away and Long Ago*. Huxley appears only for *Evolution and Ethics* but not for the *Essay on Hume*, which lays a philosophic foundation upon which any scientist can construct his own *Weltanschauung*, with a little aid from Ernst Mach and Karl Pearson. It is disappointing not to find the work that contributed most to my own reconciliation with religion—*A History of the Warfare of Science with Theology in Christendom*, by Andrew Dickson White.

Charles Darwin and the Golden Rule is an important last addition to the bibliography of William E. Ritter and helps us to place him in the history of American biology. It is scarcely an important addition to the bibliography of the relationships between science and religion.

KARL P. SCHMIDT

Chicago Natural History Museum

Prehistoric Stone Implements of Northeastern Arizona. vol. XXXIV, Rpt. No. 6. Richard B. Woodbury. Peabody Museum of American Archaeology and Ethnology, Cambridge, Mass., 1954. xiii + 240 pp. Illus. + plates. Paper, \$7.50.

In five campaigns, 1935-39, a Harvard archeological expedition led by J. O. Brew excavated at the great ruin of Awatovi and other smaller and earlier sites in the Jeddito Valley-Antelope Mesa district, Arizona, close to the modern Hopi pueblos. Awatovi was occupied from pre-Spanish times to A.D. 1700. Accounts of the remains of the 17th-century Spanish mission there and the exciting prehistoric mural paintings have already been published.

In this report, Woodbury, now professor of anthropology at Columbia University, presents the stone artifacts (and unworked special stones, of possible ritual

use, that were found in the excavations), mainly from Awatovi itself. They include polished stone axes, grooved hammers, *metates* (saddle querns) and *manos* (the upper hand-mill stones) abraders, hoes, loom-blocks, and so forth; flaked projectile points, blades, drills, and scrapers; stone ornaments (mainly turquoise and a red argillite superficially resembling Minnesota pipestone or catlinite) and small crude animal figurines; a few tubular pipes; and various other stone objects.

Woodbury's detailed presentation of this mass of material—more than 8000 specimens—is thorough, well organized, and adequately illustrated, and it reflects his careful fieldwork, with meticulous recordation, on the Awatovi expedition and also a great deal of systematic laboratory study later. For each category of stone implement, comparative notes on distribution of types in other Southwestern sites and remarks on techniques of manufacture and on probable functions or details of use accompany the conveniently summarized and tabulated descriptions.

In addition, there are excellent, although brief, general discussions of the problems of classification and interpretation and of the significance of this lithic material in understanding Hopi Indian history and acculturation. The report is a major production of great and permanent usefulness for the specialist in Southwestern archeology, and it should be of interest and value to students of anthropology and others.

ERIK K. REED

National Park Service

Cherries and Cherry Products. Roy E. Marshall. vol. V of *Economic Crops*. Z. I. Kertesz, Ed. Interscience, New York-London, 1954. xiv + 283 pp. Illus. \$8.50.

The foremost objective of this book, as stated by the author, has been to assemble a digest of the existing data and literature pertaining to the various phases of the cherry industry in North America. This has been admirably accomplished, and the information is presented in such a way that it makes the book applicable as a textbook for advanced students, as a most usable book for growers and also for those who are interested primarily in the various phases of the processing industry.

The discussions in each chapter are followed by comprehensive listings of publications dealing with investigational work in that particular field. Greatest emphasis is placed on the discussions of the cherry industry as a whole, the chemical, physiological, and biochemical changes that occur in fruits preceding and following harvest, and the harvesting, handling, and processing of the fruits.

From the standpoint of the grower and the student, excellent discussions are given on the history of the cherry and its cultivation in America, on the fruiting habits of the various species, and on the morphological structure of the buds, the flowers, and the fruit itself.

The various environmental factors of light, water, and temperatures—both summer and winter—and their effect on the responses made by cherry trees are adequately

covered. The effect of various rootstocks, pollination, general cultural practices, and disease and insect control also are considered.

The harvesting, handling, and preparation of the fruit for canning, freezing, and brining are covered in detail. Two smaller chapters are devoted to the preparation of cherry juice and dehydration of the fruit.

Excellentlly arranged, carefully indexed by both authors and subjects, and printed on good paper, this is a valuable guide for all who are interested in the cherry industry.

C. S. WALTMAN

Department of Horticulture, University of Kentucky

Development of the Guided Missile. Kenneth W. Gatland. Iliffe, London; Philosophical Library, New York, ed. 2, 1954. 292 pp. Illus. \$4.75.

K. W. Gatland, a prominent member of the British Interplanetary Society, has compiled a sort of Sears-Roebuck catalog of guided missiles, using material made available to him by numerous public relations officers and other sources on both sides of the Atlantic. The text is written in an anecdotal style, with typically English fluency, and contains as much quantitative information as security will permit.

Perusal of a book of this sort immediately causes one to ponder the difficulty of deciding when release of information accomplishes greater good for the English-speaking world than for possible military opponents. There is no doubt that this volume will do much to supply perspective for those newly approaching the guided missile art, and that it may well become a "best seller" of its type, even though the data it contains is uneven in quality and most of the vehicles described are on their way to obsolescence. To the best of my knowledge, there are relatively few inaccuracies, either of fact or principle.

The author begins with an introduction that justifies and motivates a study of missiles. He vividly describes Britain's vulnerability to guided missile attack, which is greater than that of either America or the Continent, and he also points out the ultimate desirability of satellite and space vehicles. The introduction is followed by a chapter on propulsion, in which a qualitative and somewhat superficial discussion of solid and liquid rocket and ram jet propulsors is compressed into 20 pages. The next chapter, headed "Research and development" is largely a description of NACA and RAE wind-tunnel and flight-test facilities and techniques.

In the next four chapters, a descriptive "catalog" of surface-to-air, air-to-air, air-to-surface, and surface-to-surface missiles is presented, covering 40 missiles in about 100 pages. Frequent comments on tactics, guidance techniques, design problems, warheads, and so forth, are interposed in the text.

Chapter 7, entitled, "Rockets for high-altitude research," describes vehicle specifications and research results in a more complete manner than could be attempted for weapons. The expository material concludes with two chapters on satellites and interplanetary

vehicles, respectively, which are discussed in what has become the conventional speculative manner.

A valuable feature of the book is its set of appendixes, which include 10 pages of general information on telemetry, a 10-page set of photographs of 44 prominent missiles, and a 26-page table of missiles by name and number, stating date, weight, size, range, and thrust for a goodly fraction of the 130 missiles named.

Although the volume presents something of a potpourri of facts, and I occasionally had the feeling that items of information were included because they were interesting and available, rather than because they were important or relevant, it is nevertheless a worthwhile piece of reporting and interpretation by a competent author.

HOWARD S. SEIFERT

*Guided Missile Research Laboratory,
Ramo-Wooldridge Corporation*

Relativity for the Layman. A simplified account of the history, theory, and proofs of relativity. James A. Coleman. William-Frederick, New York, 1954. xx + 131 pp. Illus. \$2.75.

This popular presentation of the theory of relativity, written by a member of the department of physics and astronomy of Connecticut College, is distinguished by a number of whimsical illustrations featuring, apparently, Elmer Fudd in the role of amateur observer and space traveler; the text reads well, too. As a boy, I had the good fortune to read Einstein's own presentation for the layman, *Relativity, the Special and the General Theory*, dated 1917, and must admit that I prefer that earlier work, although its demands on the willingness of the reader to do some hard thinking are probably greater.

Coleman's work skims over the whole theory, omitting no major aspects, and brings its applications up to date. Its weakness is that it flinches from taking the reader through the really tough conceptual problems, such as the critique of simultaneity of distant events; the twin paradox is presented but not explained. There are a few other minor objections. For those readers, however, who insist on being "entertained" while they are undergoing instruction, Coleman's little book will be valuable. I found no errors of fact or logic, and many of the illustrations are truly instructive.

PETER G. BERGMANN

*Department of Physics,
Syracuse University*

Cancer: Race and Geography. Some etiological, environmental, ethnological, epidemiological, and statistical aspects of Caucasoids, Mongoloids, Negroids, and Mexicans. Paul E. Steiner. Williams & Wilkins, Baltimore, 1954. xiii + 363 pp. Illus. \$5.

Cancer, Race and Geography is a major contribution to our knowledge of basic factors in human carcinogenesis. The document is best described in the author's own words:

The major objectives of this monograph were to use a relatively new technic for obtaining information on the importance of heredity versus environment in the etiology of human cancer by dissociating these factors, to present a statistical analysis of a large series of autopsies, to draw attention to specific deficiencies in the published ethnic and geographical data on cancer, to study the biological and morphological differences in tumors apart from frequency, to make etiological deductions from the disclosed facts, to stimulate research and new ideas on etiology, to concentrate thought on the control of cancer through prevention by reducing below threshold dosage or eliminating environmental etiological factors, and to urge the collection and reporting of racial and geographical data that might further these objectives in the future. These goals have only partially been achieved, a result which was anticipated. The great potentialities of racial and geographical studies have been explored but not fully exploited. It is hoped, however, that the next decade or two will bring these objectives much closer to realization.

This extraordinarily detailed study provides material never before available in a clear, readable, and useful form. It will stand for many years as a landmark in the development of human cancer biology.

C. P. RHODES

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Cornell University Medical College*

Foreign Policy Analysis. Feliks Gross. Philosophical Library, New York, 1954. xxiv + 179 pp. \$3.75.

This is a plea for recognition of the realities of international politics. Good foreign policy, asserts the author, must be based on an assessment of the facts of politics. The methods of science must be applied to diplomacy so that international conduct can be predicted. In his approach to analyzing foreign policy, Gross is eclectic, trying to combine the approaches of power politics, cultural determinism, and plural causation.

But after developing his argument, which could be greatly improved both in cogency and style, Gross really has said very little that is new. Good foreign policy has always searched for and analyzed the "facts" related to other nations and will continue to do so. But in designing the mosaic of policy, the facts will be blended with the aspirations, feelings, and ideas of the nations concerned. These cannot be made "scientific"—that is, rational, unless man's very nature were to be changed. In essence Gross pleads for the rule of reason in foreign affairs. This is commendable, but our method of obtaining and analyzing data is already reasonable. It is the balancing of rational data with the public passion that constitutes the crucial problem of foreign policy. Walter Lippman's *The Public Philosophy*, directed to this very point, will prove a much better analytic corrective than Gross' belabored analysis of the methods of obtaining and assessing factual data.

RALPH BRAIBANTI

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Duke University*

Excavations at Star Carr. An early mesolithic site at Seamer near Scarborough, Yorkshire. J.G.D. Clark. With chapters by D. Walker and H. Godwin; F. C. Fraser and J. E. King. Cambridge Univ. Press, New York, 1955. xxiii + 200 pp. Illus. + plates. \$11.50.

British archeologists are famous for their thoroughness, patience, precision, and perfectionism. Field archeology has been developed and cultivated in England during the last decades into a science with exacting rules and standards, demanding more of its devotees as the evidence sought became more remote and primitive. Roman remains, tangible and obvious, had been found and interpreted before, systematic field archeology had reached its climax. Prehistoric data, fugitive to the uninitiated, nowadays are located, exhumed, and analyzed with a powerful combination of skills and specialisms.

Star Carr is a mesolithic site in eastern England, more precisely a camping-ground of three to four families, occupied seasonally in a period that can be recognized as an early postglacial phase, slightly earlier than the so-called "Maglemosian" sites in Denmark. The camping-site was established on a birchwood platform laid over the surface of a reed-swamp. In water-logged condition, many remnants of the daily life and toil of the inhabitants were salvaged; flint, antler, and bone tools and weapons; the world's earliest wooden paddle; a few beads, humble beginnings of mesolithic art; and striking evidence of the use of stag frontlets for magic or ritual purposes.

The restraint with which the evidence is presented almost suppresses the miraculous aspect of the fact that conditions of life in eastern England about 7500 B. C. (the carbon-14 date established by Libby) can be so nearly understood and interpreted. The excavation report is also a textbook of methodology and a delight to the perfectionist. It is supplemented by a chapter on "Lake-stratigraphy, pollen-analysis and vegetational history," by D. Walker and H. Godwin; and one on "Faunal remains," by F. C. Fraser and J. E. King. These contributions form as integral a part of the book and its conclusions as do the strictly anthropocentric chapters.

Early stages of human progress can be reconstructed only by a team of scientific specialists. At present such a team is at work in Iraq to trace the earliest stages of food-producing communities. The Star Carr publication fully demonstrates the potentialities of the combined method. The form of the book equals its contents in excellence.

MACHTELD J. MELLINK

*Department of Classical Archaeology,
Bryn Mawr College*

Auxins and Plant Growth. A. Carl Leopold. Univ. of Calif. Press, Berkeley, 1955. xi + 354 pp. Illus. \$5.

This book was written primarily for the agricultural research worker and is intended to provide a brief review of those experiments in "fundamental" plant physiology from which stem the many commercial applications of auxins (plant-growth substances). The choice, inevitable when writing a book about a very active research field, of including only data that are well established (and thus are somewhat out of date upon publication) or of including the most recent work possible has been settled for the latter alternative. A. C. Leopold has included a surprising amount of very recent research. The book starts with a particularly useful long chapter on techniques for obtaining and measuring auxins. The next 120 pages cover "Fundamentals of auxin action," with the last 120 pages discussing "Auxins in agriculture." Papers written in English make up most of the bibliography.

As one would expect from Leopold's own research interests, the biochemical side of physiology is emphasized. Within the limits that he has set for himself, Leopold has written a good summary of the field. A particularly attractive feature of the book is the liberal use of uniform, well-designed, and well-labeled graphs, all of which were apparently drawn for this book.

WILLIAM P. JACOBS

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Books Reviewed in SCIENCE

1 July

Textbook of Physics, R. Kronig, Ed. (Interscience; Pergamon Press.) Reviewed by G. C. Danielson.

Modern Aspects of Electrochemistry, J. O'M. Bockris, Ed. (Academic Press; Butterworths). Reviewed by W. J. Hamer.

La Cybernétique, Paul Cossa (Masson). Reviewed by J. Rothstein.

International Review of Cytology, vol. III, G. H. Bourne and J. F. Danielli, Eds. (Academic Press). Reviewed by M. J. Kopac.

Principles of Internal Medicine, T. R. Harrison et al., Eds. (Blakiston). Reviewed by R. A. Kinsella.

Laboratory Techniques in Rabies, World Health Organization (Columbia Univ. Press). Reviewed by J. P. Fox.

The Chemistry of Petroleum Hydrocarbons, vol. I. Benjamin T. Brooks, Cecil E. Boord, Stewart S. Kurtz, Jr., Louis Schmerling, Eds. (Reinhold). Reviewed by G. Egloff.

Inventories of Apparatus and Materials for Teaching Science, vol. III, *Technical Colleges*; pt. 4, *Electrical Engineering*, UNESCO (Columbia Univ. Press). Reviewed by L. A. Harris.

Probleme und Beispiele biologischer Regelung, R. Wagner; *Einführung in die biologische Registriertechnik*, Herbert Klensch (Thieme). Reviewed by E. Gellhorn.

The Why of Chemistry Problems, Fred B. Eiseman, Jr. (Educational Publ.). Reviewed by E. F. Degering.

Chemical Pathways of Metabolism, vol. I. David M. Greenberg, Ed. (Academic Press). Reviewed by J. F. Taylor.

Proceedings of the Thirty-third Annual Meeting, Highway Research Board, Fred Burggraf and Walter J. Miller, Eds. (National Academy of Sciences-National Research Council). Reviewed by B. S. Coffman.

8 July

Clinical versus Statistical Prediction, Paul E. Meehl (Univ. of Minnesota Press). Reviewed by L. Herrera.
Pharmacology in Medicine, Victor A. Drill, Ed. (McGraw-Hill). Reviewed by H. M. Jones, Jr.
Standard Values in Nutrition and Metabolism, E. C. Albritton, Ed. (Saunders). Reviewed by H. G. Day.
Human Heredity, James V. Neel and William J. Schull (Univ. of Chicago Press). Reviewed by C. N. Herndon.

An Introduction to Molluscan Ecology, Alan Mozley (Lewis). Reviewed by W. J. Clench.

Fishes of the Western North Atlantic, Henry B. Bigelow and William C. Schroeder (Sears Foundation for Marine Research, Yale Univ.). Reviewed by L. P. Schultz.

Feeding Poultry, Gustave F. Heuser (Wiley; Chapman & Hall). Reviewed by R. L. Bryant.

Linear Equations in Applied Mechanics, H. F. P. Purday (Interscience; Oliver and Boyd). Reviewed by M. Golomb.

Le Magnétisme des Corps Célestes, vols. 1 and 2, parts II and III of *Physique Cosmique*, A. Dauvillier (Hermann). Reviewed by A. G. McNish.

Sponsored Research Policy of Colleges and Universities, Committee on Institutional Research Policy (American Council on Education). Reviewed by H. B. Steinbach.

Introduction to the Theory of Neutron Diffusion, vol. I, K. M. Case, F. de Hoffman, G. Placzek (Los Alamos Scientific Lab.). Reviewed by B. T. Feld.

Structure of Molecules and Internal Rotation, San-Ichiro Mizushima; Eric Hutchinson, Ed. (Academic Press). Reviewed by P. C. Cross.

The Structural Chemistry of Proteins, H. D. Springall (Academic Press; Butterworths). Reviewed by M. Levy.

A Short Textbook of Colloid Chemistry, B. Jirgensons and M. E. Straumanis (Wiley; Pergamon Press). Reviewed by L. H. Reyerson.

15 July

The Proteins, vol. II, pt. B, Hans Neurath and Kenneth Bailey, Eds. (Academic Press). Reviewed by F. W. Putnam.

Strength and Resistance of Metals, John M. Lessells (Wiley; Chapman & Hall). Reviewed by G. M. Sinclair.

Isotope Geology, Kalervo Rankama (McGraw-Hill; Pergamon Press). Reviewed by E. W. Berry.

Vitamins and Hormones, vol. XII, Robert S. Harris, G. F. Marrian, Kenneth V. Thimann, Eds. (Academic Press). Reviewed by B. F. Chow.

General Chemistry, W. Norton Jones, Jr. (Blakiston). Reviewed by A. B. Garrett.

Perspectives and Horizons in Microbiology, Selman A. Waksman, Ed. (Rutgers Univ. Press). Reviewed by A. W. Bernheimer.

Psychological Statistics, Quinn McNemar (Wiley; Chapman & Hall). Reviewed by G. F. J. Lehner.

Hypertension: Humoral and Neurogenic Factors, G. E. W. Wolstenholme and Margaret P. Cameron, Eds. (Little, Brown). Reviewed by A. C. Corcoran.

22 July

Advances in Carbohydrate Chemistry, vol. 9, M. L. Wolfrom, R. S. Tipson, E. L. Hirst, Eds. (Academic Press). Reviewed by M. P. Gordon.

L'Analyse Spectrale Quantitative par la Flamme, R. Mavrodineanu and H. Boiteux (Masson). Reviewed by B. F. Scribner.

The Chemistry of Portland Cement, Robert Herman Bogue (Reinhold). Reviewed by H. H. Steinour.

La Végétation de Kaniama (Entre-Luishi-Lubilaish, Congo Belge), William Mullenders (Institut National pour l'Etude Agronomique de Congo Belge). Reviewed by A. Cronquist.

Deterioration of Materials, Glenn A. Greathouse and Carl J. Wessel, Eds. (Reinhold). Reviewed by F. E. Dickinson.

Contributions to the Theory of Partial Differential Equations, L. Bers, S. Bochner-F. John, Eds. (Princeton Univ. Press). Reviewed by M. Golomb.

Mosquitoes: Their Bionomics and Relations to Disease, William R. Horsfall (Ronald Press). Reviewed by M. Bates.

Degradation of Vinyl Polymers, H. H. G. Jellinek; vol. III of *Physical Chemistry*, Eric Hutchinson, Ed. (Academic Press). Reviewed by L. A. Wall.

Advances in Food Research, vol. V, E. M. Mrak and G. F. Stewart, Eds. (Academic Press). Reviewed by H. G. Day.

Connective Tissues, Charles Ragan, Ed. (Josiah Macy, Jr. Foundation). Reviewed by J. A. Arcadi.

29 July

Colchicine—in Agriculture, Medicine, Biology and Chemistry, O. J. Eigsti and Pierre Dustin, Jr. (Iowa State College Press). Reviewed by C. Chandler.

A Manual of Paper Chromatography and Paper Electrophoresis, R. J. Block, E. L. Durrum, G. Zweig (Academic Press). Reviewed by H. B. Bensusan.

Annual Review of Nuclear Science, vol. 4, James G. Beckerley, Martin D. Kamen, Leonard I. Schiff, Eds. (Annual Reviews). Reviewed by D. C. Peaslee.

Les Bactéries Lysogènes et la Notion de Provirus, F. Jacob (Masson). Reviewed by M. H. Adams.

Differential and Integral Calculus, Harold M. Bacon (McGraw-Hill). Reviewed by M. F. Roskopf.

Advanced Mathematics for Engineers, ed. 3, H. W. Reddick and F. H. Miller (Wiley). Reviewed by M. Rees.

Magnetic Amplifiers, H. F. Storm (Wiley; Chapman & Hall). Reviewed by G. J. Murphy.

Physicochemical Calculations, E. A. Guggenheim and J. E. Prue; J. DeBoer, H. Brinkman, H. B. G. Casimir, Eds. (Interscience; North-Holland). Reviewed by A. Tobolsky.

Enzymologie, Otto Hoffmann-Ostenhof (Springer). Reviewed by G. W. E. Plaut.

The Biology of Man, John S. Hensill (Blakiston). Reviewed by L. P. Johnson.

Statistical Problems of the Kinsey Report on Sexual Behavior in the Human Male, W. G. Cochran, F. Mosteller, J. W. Tukey (American Statistical Assoc.). Reviewed by Q. McNemar.

Health Careers Guidebook (National Health Council). Reviewed by M. O. Lee.

Genetica Medica, Luigi Gedda, Ed. (Edizioni dell' Istituto Gregorio Mendel). Reviewed by A. A. Buzzati-Traverso.

Nuclear Physics, Alex E. S. Green (McGraw-Hill). Reviewed by J. J. Kraushaar.

New Books

- Diseases of the Nervous System.** Described for practitioners and students. F. M. R. Walsche. Williams & Wilkins, Baltimore, ed. 8, 1955. 357 pp. \$7.
- Grain Crops.** Harold K. Wilson. McGraw-Hill, New York-London, ed. 2, 1955. 396 pp. \$6.50.
- Experiments in Organic Chemistry.** Louis F. Fieser. Heath, Boston, ed. 3, 1955. 359 pp. \$5.25.
- Flight Handbook.** A complete introduction to aviation. Maurice A. Smith, Ed. Philosophical Library, New York and Iliffe, London, ed. 5, 1954. 282 pp. \$6.
- An Introduction to Stochastic Processes with Special Reference to Methods and Applications.** M. S. Bartlett. Cambridge Univ. Press, New York 1955. 312 pp. \$6.50.
- Introductory Applied Physics.** Norman C. Harris and Edwin M. Hemmerling. McGraw-Hill, New York-London, 1955. 729 pp. \$6.75.
- Electrons, Atoms, Metals and Alloys.** William Hume-Rothery. Philosophical Library, New York and Iliffe, London, ed. 2, 1955. 387 pp. \$10.
- Mathematical Foundations of Quantum Mechanics.** John Von Neumann Trans. by Robert T. Beyer. Princeton Univ. Press, Princeton, 1955. 445 pp. \$6.
- The Nitrogen Metabolism of Micro-organisms.** B. A. Fry. Wiley, New York; Methuen, London, 1955. 166 pp. \$2.
- Atomic and Nuclear Physics.** Robert S. Shankland. Macmillan, New York, 1955. 529 pp. \$7.75.
- Protective Coatings for Metals.** R. M. Burns and W. W. Bradley. Reinhold, New York, ed. 2, 1955. 643 pp. \$12.
- Mechanisms of Microbial Pathogenicity.** 5th symposium of the Society for General Microbiology held at the Royal Institution, London, April 1955. J. W. Howie and A. J. O'Hea, Eds. Cambridge Univ. Press, New York, 1955. 333 pp. \$5.
- The Skin, a Clinicopathologic Treatise.** Arthur C. Allen. Mosby, St. Louis 1954. 1048 pp. \$25.
- Fundamentals of Plant Science.** A laboratory manual. G. W. Prescott and J. C. Elliott. Burgess, Minneapolis, 1955. 271 pp. \$4.
- Neuropharmacology.** Transactions of the First Conference 26-28 May 1954, Princeton, N.J. Harold A. Abramson, Ed. Josiah Macy, Jr. Foundation, New York, 1955. 210 pp. \$4.25.
- The Hypophyseal Growth Hormone, Nature and Actions.** Richmond W. Smith, Oliver H. Gaebler, and C. N. H. Long, Eds. Blakiston Div., McGraw-Hill, New York-London, 1955. 576 pp. \$12.
- Current Trends in Psychology and the Behavioral Sciences.** J. T. Wilson, C. S. Ford, B. F. Skinner, G. Bergmann, F. A. Beach, and K. Pribram. Univ. of Pittsburgh Press, Pittsburgh, 1954. 142 pp. \$4.
- Manganese.** Metallurgy of the rarer metals, No. 3. A. H. Scully. Academic Press, New York; Butterworths, London, 1955. 305 pp. \$6.50.
- Thermodynamics.** From the classic and generalized standpoints. Joseph Louis Finck. Bookman, New York, 1955. 224 pp. \$7.50.
- Beta- and Gamma-Ray Spectroscopy.** Kai Siegbahn, Ed. Interscience, New York; North-Holland, Amsterdam, 1955. 959 pp. \$20.
- Steinmetz: Maker of Lightning.** Sigmund A. Lavine. Dodd, Mead, New York, 1955. 241 pp. \$3.
- Your Career in Physics.** Philip Pollack. Dutton, New York, 1955. 127 pp. \$2.75.
- Scientific Writing.** Meta R. Emberger and Marian R. Hall. W. Earl Britton, Ed. Harcourt, Brace, New York, 1955. 468 pp.
- Photosynthesis.** Monograph on biochemical subjects. Robert Hill and C. P. Whittingham. Methuen, London; Wiley, New York, 1955. 165 pp. \$2.
- Tables of Sines and Cosines for Radian Arguments.** NBS Applied Math. Ser. No. 43. National Bureau of Standards, Washington 25, 1955 (Order from Supt. of Documents, GPO, Washington 25). 275 pp. \$3.
- Some Fundamentals of Combustion.** Gas Turbine Ser., vol. 2. D. B. Spalding. Academic Press, New York; Butterworths, London, 1955. 250 pp. \$7.50.
- Polarographic Techniques.** Louis Meites. Interscience, New York-London, 1955. 317 pp. \$6.
- The Botany of Cook's Voyages and Its Unexpected Significance in Relation to Anthropology, Biogeography and History.** vol. 14, No. 5/6. Elmer Drew Merrill. Chronica Botanica, Waltham, Mass.; Stechert-Hafner, New York, 1954. 119 pp. \$4.75.
- Practical Medical Mycology.** Edmund L. Keeney. Thomas, Springfield, Ill., 1955. 145 pp. \$4.50.
- General Physics.** Oswald Blackwood and William Kelly. Wiley, New York; Chapman & Hall, London, ed. 2, 1955. 704 pp. \$6.75.
- Clinical Endocrinology.** For practitioners and students. Laurence Martin and Martin Hynes. Little, Brown, Boston, ed. 2, 1954. 253 pp. \$5.50.
- The Elements of Astronomy.** A nonmathematical textbook for use as an introduction to the subject in colleges, universities, etc., and the general reader. Edward Arthur Fath. McGraw-Hill, New York-London, ed. 5, 1955. 369 pp. \$5.50.
- Unified Algebra and Trigonometry.** Elbridge P. Vance. Addison-Wesley, Cambridge, 1955. 354 pp. \$4.50.
- Principles of Medical Statistics.** A. Bradford Hill. Oxford Univ. Press, New York, ed. 6, 1955. 314 pp. \$4.
- Outside Readings in Geography.** Fred E. Dohrs, Lawrence M. Sommers, and Donald R. Petterson. Crowell, New York, 1955. 805 pp. \$2.95.
- Electrochemistry in Biology and Medicine.** Theodore Shedlovsky, Ed. Wiley, New York; Chapman & Hall, London, 1955. 369 pp. \$10.50.
- Introduction to the Philosophy of Being.** George P. Klubertanz. Appleton-Century-Crofts, New York, 1955. 300 pp. \$3.
- Basic Lubrication Practice.** Allen F. Brewer. Reinhold, New York, 1955. 286 pp. \$6.75.
- Autoradiography in Biology and Medicine.** George A. Boyd. Academic Press, New York, 1955. 400 pp. \$8.80.
- Better Health for Your Children.** I. Newton Kugelmass. McGraw-Hill, New York-London, 1955. 341 pp. \$4.50.
- Morbidity in the Municipal Hospitals of the City of New York.** Marta Fraenkel and Carl L. Erhardt. Russell Sage Foundation, New York, 1955. 229 pp. \$4.50.
- The House on Nauset Marsh.** Wyman Richardson. Norton, New York, 1955. 223 pp. \$3.75.
- Biochemistry: An Introductory Textbook.** Felix Haurowitz. Wiley, New York; Chapman & Hall, London, 1955. 485 pp. \$6.75.
- Capricorn Road.** Francois Balsan. Trans. by Pamela Search. Philosophical Library, New York, 1955. 252 pp. \$4.75.

LETTERS

What Is Wrong with Pl?

I read with interest C. E. Whitmore's article, "The language of science" [*Sci. Monthly* 80, 185 (Mar. 1955)]. It reminded me of a confession as to the *raison d'être* of a particular scientific symbol that I heard some time ago. Perhaps Whitmore will enjoy the confession.

At the October 1946 meeting of the California Section of the American Chemical Society, G. T. Seaborg spoke on "Plutonium and nuclear energy." During the question period following his talk, a member of the very large audience rose and questioned Seaborg about like this: "We all know that the symbol for lead is Pb, derived from the Latin *plumbum*, and that the symbol for platinum is Pt. With the discovery of plutonium, we find the symbol Pu assigned to it. I would like to know, what is the matter with Pl? It seems a perfectly obvious choice for each of these, yet it has been ignored three times in a row."

I think I can remember Seaborg's reply almost exactly. He said, "I don't know why Pl was not chosen for either lead or platinum. We were aware that it would be a reasonable symbol for plutonium but then we thought of Pu and we just could not resist the temptation."

J. T. MITCHELL

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Dandelions

The cover photograph of the dandelion head [*Sci. Monthly* 80 (Apr. 1955)] prompted me to submit one of *Tragopogon* fluffs. Student-teachers and their pupils at Macalester College in St. Paul, Minnesota, were entertained by this photograph. The dandelion fruit the children say, is "white fluff" made by their "golden puff."

The *Tragopogon pratensis* (Linn.) flower in bloom looks like a green star with eight rays from 2 to 2½ in.



in diameter, on which is superimposed a yellow star of 10 or more rays 1½ to 2 in. in diameter. The pistils and stamens in this yellow star form a rich yellow center, which the children compare with their "golden puff."

MARY J. HAGG

Macalester College, St. Paul, Minnesota

More on Air Pollution

Our article "Some scientific aspects of the urban air pollution problem," [*Sci. Monthly* 81, 10 (July 1955)], in the interest of scientific accuracy, deserves a few words of explanation and amendment. It is felt that the editorial note at the beginning does not make sufficiently clear the fact that most of the material in this article was taken bodily from publishable papers presented at the AAAS symposium by the six other participants. Tables 2 and 3, for example, and other material, should be credited to the Los Angeles County Air Pollution Control District.

In attempting to condense the six symposium papers, we do not wish to assume credit for all of these important data. There is a great deal more information here than we can vouch for, and some statements we cannot fully support at this time. For example:

Smog gases, including ozone, are said to be oxidizing substances that are formed by the action of sunlight on dilute vapors of organic compounds, such as hydrocarbons.

Presumably this particular contributor intended to include oxides of nitrogen as an essential reactant.

The control of hydrocarbons in automobile exhaust is accomplished. . . .

This should read "might be accomplished."

Another method to eliminate hydrocarbons at the source is the use of alternate fuels. . . .

Again, this should read "might be the use of alternate fuels."

On page 17, under "Aldehydes," the second sentence in that paragraph should read, "They are produced by engine exhaust. . . ."

In Fig. 2 the pictures are reversed.

In the second sentence of the paper, we would not claim that polluted air "stagnates as it moves"—it does one or the other.

LAUREN B. HITCHCOCK
HELEN G. MARCUS

Air Pollution Foundation,
Los Angeles, California

Erratum: In the article "Human background of Pacific science," by Alexander Spoehr, which appeared in the July issue, an unfortunate last-minute mistake occurred on page 8 of some copies. The legend for the pictures should read: "(Left) Boatworks at Ponape. (Right) Weighing copra in a Ponape warehouse for export."

ASSOCIATION AFFAIRS

Traveling Science Libraries for Small High Schools

To assist high-school students to learn more about science, and to interest some of them in becoming scientists, the AAAS will start in the fall a program of sending traveling science libraries to a selected list of small high schools. Plans were developed co-operatively with the U.S. Office of Education and the National Science Foundation, which has made a grant to the Association to cover the costs. (This program is in addition to the science Teaching Improvement Program (see page 109 of this issue).

The lack of appropriate reading material in small high schools deprives a large segment of the student population of the opportunity to learn what science is like and what scientists do. As a result, many young people with potential interest in careers in science fail to capitalize on their talents. Furthermore, many lack motivation to continue their education. It is also important that those not contemplating science careers be informed citizens in science as well as in other cultural areas.

It is the hope of the sponsors that the traveling science libraries will serve the following purposes: (i) develop greater interest on the part of high-school students in reading books on science and about scientists; (ii) make available to students a larger fund of factual information from the great scientific storehouse; (iii) develop a sounder basis for the choice of a career in science; (iv) afford science teachers an opportunity to extend their scientific reading; (v) stimulate an interest on the part of schools in purchasing similar collections of books for their libraries.

Selection of participating schools. Ten reasonably limited geographic areas, with varying cultural and educational characteristics, will be selected. In each area six senior high schools in the smaller communities will be chosen. The schools will be asked whether they wish to avail themselves of the traveling libraries; only those offering full cooperation will be given the privilege of participating.

Selection of books. The collection is to consist of 120 books covering the major fields, such as agriculture, anthropology, astronomy, botanical sciences, biology, chemistry, engineering, geology and geography, history and philosophy of science, mathematics, meteorology, medical sciences, physics, psychology, and zoological sciences. The selection will be based on the suggestions and recommendations of individuals, committees, and organizations representing the various scientific disciplines. The books to be included must contain au-

thoritative scientific information written in a style that will interest high-school students. Textbooks or research monographs will not be included. In general the books will be chosen because they may be read and understood by persons with a limited background in science. A few, however, will be at a level to challenge the better students.

Plan of operation. The 120 books will be divided into 6 units of 20 each. Each unit will be fitted into an attractive case that can be used both for shipping and display. Each participating school will be permitted to retain each of the 6 units of 20 books for 4 class weeks. The first unit will arrive at each school about 1 Oct. 1955. All units will leave the schools approximately 1 May 1956.

Since the project is experimental and is supported by very limited funds, the libraries can be sent only to the selected schools. Voluntary applications for participation cannot be honored.

A brochure describing the collection and its use and containing a brief résumé of each book, will be sent to the school librarian and to the science teachers at each school before the books arrive. A copy of the résumé will also be inserted in the front of each book.

The teachers and the school librarian will be asked to make known the availability of the books and to encourage students to read them. Class assignments may be used to stimulate interest in reading the books available during a given period, but the teachers in the participating schools will be requested not to make required reading of the volumes in the libraries. Nothing should be done to kill spontaneity. Each school will be expected to take all steps practicable to insure that as many students as possible have an opportunity to read the books during the time each unit is available.

Direction. Hilary J. Deason has been appointed director of the traveling science libraries program. Born in Utah and educated at the University of Michigan, he was granted his Ph.D. degree in 1936. After several years of work in fishery biology and limnology on the Great Lakes for the U.S. Bureau of Fisheries, he served as an administrator in the Fish and Wildlife Service in Washington. There he was responsible for the program of technical cooperation with foreign countries and training programs for foreign students. He was a member of the former Interdepartmental Committee on Scientific and Cultural Cooperation of the Department of State and has served as a delegate to various inter-

national conferences on conservation of biological resources and technical cooperation.

Recommendations from scientists, librarians, and teachers on books for the library list will be welcome and should be sent to Deason at the AAAS office. The selected books and a longer supplementary list will be given wide publicity.

JOHN A. BEHNKE

Director Named for AAAS Science Teaching Improvement Program

The announcement that my colleague, John Mayor, has agreed to be the first director of the AAAS Science Teaching Improvement Program is very welcome news. The program to increase the number of well-qualified science and mathematics teachers is the culmination of the work of many groups, centering in that of the Cooperative Committee on the Teaching of Science and Mathematics. This committee has been in existence since 1941 and, since the close of World War II has been chaired by Karl Lark-Horovitz of the department of physics, Purdue University; Morris Meister, principal of the Bronx High School of Science; and John Mayor of the departments of mathematics and education, University of Wisconsin.

To carry out the program described at the beginning of this issue entails the cooperation of many persons, but it also depends upon some one person to see that it goes forward on all fronts, to coordinate the efforts of others, and to give administrative direction to the whole. As is the case for every important post, this person should have intelligence, vigor and character, but for this particular position he also must be both a scientist and a person who has had intimate association with secondary education. Mayor, to a remarkable degree, satisfies these conditions.

Mayor is now 49 years old. He received his bachelor's degree from Knox College, his master's degree from the University of Illinois, and in 1933 his Ph.D. in mathematics from the University of Wisconsin, his field of specialization being geometry. During his graduate work at Wisconsin, he taught as a graduate assistant and after receiving his doctorate, continued at the university and its Milwau-

kee Extension Division until 1935, when he became chairman of the department of mathematics at Southern Illinois University. He returned to Wisconsin in 1947 as associate professor of mathematics and education in charge of the training of mathematics teachers. He was promoted to professor in 1951, again in both the department of mathematics and the department of education; he also served as chairman of the department of education. During 1954-55 he has been acting as dean of the School of Education, still retaining his professorship in mathematics. His teaching program has included courses in pure mathematics, courses in the teaching of mathematics for high-school teachers, and direction of the program of mathematics in the Wisconsin High School where he supervised practice teaching in the field of mathematics.

By nature Mayor is a gifted teacher; yet he has always shown, both by example and precept, that even the exceptionally able teacher can improve through deepening his knowledge of his subject and studying the methods for presenting it—illustrating the fact that a man reaches excellence only through a combination of talent and education. He has written many articles, some of them research papers in mathematics, some mathematical exposition, and still others concerned with the teaching of mathematics. Mayor has a capacity for hard work and an enthusiasm that makes him enjoy it. This enthusiasm is not so much an ebullience of spirit as a keen desire to help attain goals that he believes are important. This not only has made him an outstanding success as a teacher at the university but has led him to serve on many committees for the improvement of scientific and mathematical education, to serve as officer of professional societies, including the presidency of the National Council of Teachers of Mathematics, to speak before state and local groups throughout the country, to serve as chairman of the Cooperative Committee, and now to be charged with carrying its program forward. I know of no one who would be more likely to carry such a program forward with vigor or to secure from others the cooperation necessary to its success.

MARK H. INGRAHAM

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